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Description of a new Machine for curing diſtorted Limbs.

[With an Engraving.]

Communicated by Mr. ROBERT WATT, Surgeon, in PAISLEY.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

IN the Preface to the firſt volume of your Medical and Phyſical Journal, you mention your not being ſo fortunate in the departments of Anatomy and Surgery, as in the other branches of medical ſcience. This may be owing to theſe two having arrived nearer to perfection than any of the reſt; or, it may be owing to the negligence of ſome practitioners, in not communicating freely to the world whatever chance may have thrown into their way. Others there are, who, actuated by a more ſelfiſh and degrading principle, wiſh carefully to conceal from the public, as far as poſſible, any improvement or diſcovery they may have made, in order that none but themſelves may reap the benefit. The idea, however, is abſurd, and equally injurious to the perſon himſelf, as to the public; for it frequently happens, that the more freely a man communicates his improvements and diſcoveries to the world, the more generouſly will he be rewarded. At any rate, ſuch perſons feel a reward in their breaſt, of which none but themſelves can have any conception. It ſhall, therefore, be my ambition, through the excellent medium of your Journal, to communicate to the public whatever hints or improvements I may in future be able to make.

There is one malady to which the human race is not infrequently ſubjected, and for which, ſo far as I know, ſurgery has made little or no proviſion, namely, the diſtorted limbs of infants. What I chiefly refer to, are thoſe two ſpecies of diſtortion

distortion, known by the terms varus and valgus. Whether the fault has been owing to the parents of these unfortunate children, in not having applied in time; or, whether it has been owing to a deficiency in the medical assistance they have employed, it is, perhaps, impossible to determine; but certain it is, that numbers of both sexes have remained in this unhappy situation for life: whereas, in this part of the country, I have not seen a single instance of one who has not been cured of the disease.

It is more than probable, that in many of these cases the fault has been owing to the parents. They, as if they looked upon themselves as the cause of the misfortune, and of course blameable, carefully conceal the matter as long as possible, not only from the public, but frequently even from their family practitioner. It is also a commonly received opinion, that the regular bred surgeon can afford little or no assistance in such uncommon disorders as those of distorted limbs. Therefore, if application is made at all, it is made to itinerants and quacks; who, after having made what they can of the credulous parents, generally leave the unhappy victims in a worse state than that in which they found them.

This disorder may be occasioned by different causes; by a morbid state of the bones, or contraction of the muscles; by original mal-conformation, or wrong position in the womb. But from whatever cause the distortion may proceed, the reducing them to the natural position must be accomplished by nearly the same means; that is, by judicious pressure upon the convex side. In some cases the disorder lies in the ankle joint, while the leg and knee are perfectly natural; in others, and perhaps, the greatest number, it is occasioned by a bending of the bones of the leg, by which the toes are turned either out or in, according as the bones are bent to the one side or to the other.

A case of the first kind, where the disorder lay principally in the ankle joint, occurred to me lately. J. Wardrop, a child about seven weeks old, had her feet so distorted, that the toes of the one pointed directly to the ankle of the other. Upon the external ankle of the left, and down the back of the foot, there was a considerable rising, with a corresponding hollow in the other side, which showed that the bones of the ankle joint were shifted completely out of their place. The right foot had more of the natural shape, but, with regard to the directions of the toes, it was as much distorted as the other. The soles of both were considerably turned, so that, when walking, she would have gone directly upon the outsides of her feet.

From

From her birth till application was made to me, various attempts had been made to correct the deformity; ſhoes, boots, and bandages of all kinds had been tried, but without ſucceſs. Some of them it was found impoſſible to retain for any length of time, or if retained, they hurt the feet ſo much, that they were under the neceſſity of removing; others, and that the greateſt number, if they did no harm, they did as little good. When ſhe was firſt put under my care, as I had never ſeen a caſe of the ſame kind treated before, I began to turn over every volume upon ſurgery I could lay my hands on; but finding nothing to my purpoſe ſave in general terms, I began to think for myſelf, when I fell upon the following contrivance, which I am happy to ſay, has anſwered the purpoſe extremely well.

Figure I. repreſents the machine containing the limbs of the child. AAAA, the legs and feet of the machine, which ought to be made of white iron, hardened leather, or ſome other firm ſubſtance, ſtrong enough to retain the limbs in a proper poſition. BB, two pieces of ſoft leather fixed to the inſide of the legs of the machine, laced upon the foreſide of the limbs of the child to retain them in their place: the toes ought to be left uncovered, by this means we can judge more accurately of the poſition of the feet. CC, two pieces of iron with joints in the middle; theſe connect the limbs together, and by the joints allow the toes to be ſeparated from one another, or brought nearer, according as the caſe may require. DD, two curved pieces of iron fixed to the toes of the machine, the one having a long ſlit, the other a ſcrew, by which means the toes can be fixed in any poſition we pleaſe.

To prevent the child's feet from being hurt by preſſure againſt the inſide of the machine, let them be rolled with a ſoft flannel roller from the toes up to the knee, and frequently anointed with ſome emolient liniment. At firſt, the machine ſhould be applied only for a few hours at once; but after ſome time, it may be kept on for two or three days without doing the leaſt injury to the child's feet, and with conſiderably more advantage than when it is frequently removed. In the above caſe, at one time the machine was kept on without being changed for upwards of four days; and I obſerved, that in this interval they made more progreſs than ever they had done before in the ſame time, when the machine was changed daily. To keep the child quiet while we were putting on the inſtrument, and through the night, I found it neceſſary to uſe a gentle opiate once in the twenty-four hours. This, however, did no hurt to the child's health; and as ſoon as the operation was finiſhed, we gave it up entirely.

In turning out or in the toes it ſhould be done by degrees, and not all at once: this may be eaſily managed by means of the ſcrew H. By making a ſmall advance every day, a ſhort time will bring them to the place we wiſh. And here it may be proper to obſerve, that whether the feet are turned out or in, it will be neceſſary to bring them about with the machine a little farther than we wiſh them to ſtand, for after it is finally removed they naturally recede a little.

I need ſcarcely obſerve too, that though the one foot only ſhould be diſtorted, it will be neceſſary to uſe a double machine, that is, a machine with two legs, one for the ſtraight limb as well as one for the crooked: for it is impoſſible to retain the diſtorted one in a proper poſition without connecting it to the other. In this way the cure will be done with the greateſt eaſe, and no injury whatever will be done to the ſound limb. In the caſe above related, although the machine was kept on for nearly two months, yet there was not a ſingle ſcratch or bruiſe upon her whole limbs, and all the while ſhe grew, and was as healthy as ever ſhe was, either before or ſince.

When I firſt thought of this inſtrument, I had no idea of its being applied to any other caſe but the *vari* and *valgi*, and theſe only when the diſorder happened to lie in the ankle joint. But I have ſince thought, that by a ſmall alteration, it may be applied to all the different kinds of diſtorted limbs with conſiderable eaſe and advantage.

Figure II. repreſents the half of the machine, in every reſpect ſimilar to the one above deſcribed, ſave only, that one of its ſides from the heel up is removed, and in place of it a ſpring, the under end of which is fixed at E, to the ſide of the heel; the other, by means of the ſtrap F, to the top of the machine. The convex ſide of the ſpring, ſtuffed and covered with ſoft leather, being applied to the convex ſide of the limb, by means of the ſtrap F. any degree of preſſure can be made which the practitioner may judge neceſſary. The ſide of the leg of the machine oppoſite the ſpring, muſt have a head as repreſented at G, to reſt againſt the outer or inner condyle of the femur, according as the caſe may require. If the bones are bent forward, as is often the caſe, by fixing the ſpring upon the foreſide, and cauſing it to preſs upon the convexity of the limb, they may be cured in the ſame manner as above directed. By uſing a ſpring inſtead of ſtraps round the limb, as directed by Mr. Benjamin Bell, a more uniform and effectual preſſure can be made. With regard to the ſtrength of the ſpring, no general rule can be given; this muſt depend entirely upon the age of the patient, or the degree of force required to ſtraighten the limb.

Figure

Figure III. reprefents one leg of the machine without the child's limb, having a fpring fixed to the one fide of the heel, as above directed. At LL and II, there muft be two rows of fmall holes, to which the leather which furrounds the child's limbs muft be fixed. K reprefents a fcrew with which the fpring may be fixed inftead of the ftrap F. I am,

GENTLEMEN,

Your humble fervant,

Paifley, April 3, 1800. R. WATT.

P. S. At the time when the above was written and ready to be fent off, J. Wilfon, a child about eight days old, having her feet diftorted in the fame manner as above defcribed, but confiderably worfe, was put under my care; I treated her in the fame manner and with equal fuccefs. In the courfe of two months fhe was completely cured, and her feet are now as natural and well-fhaped as any other child's of the fame age. At the firft, the offa calcium feemed either to be extremely fmall, or completely wanting; but after applying the machine for a few weeks, they grew and became perfectly well formed. The protuberances too upon the outfides of her feet, produced by the difplacing of the metatarfal bones, difappeared, and the hollow below the internal ankle filled up. From the fuccefs in treating thefe two cafes, I am inclined to think that there is no kind of diftorted limb, however formidable it may appear, if taken in time, but may be cured by the fame means.

Some Remarks on the Medicinal Properties of Ledum Paluftre.

By WILLIAM GEORGE MATON, M. B. F. R. S. &c.

NOTWITHSTANDING the multitude and variety of the articles already received into the Materia Medica, and the confequent hefitation with which cautious practitioners adopt the exhibition of lefs noted medicines, the healing art cannot furely be provided with too many refources; nor will the fcientific phyfician ceafe to feek in the treafures of Nature for new fubftances fubfervient to his profeffion. But, without prefuming to recommend any fubftances which have very recently been difcovered to poffefs medical properties, I wifh only to bring into general notice, fome which have long been known,

but

but which do not ſeem to have been ſubjected to experiment in this country.

Of the diſeaſes which moſt obſtinately reſiſt the ordinary means of cure, none are leſs productive of credit to the practitioner, or more haraſſing to the patient, than thoſe which are commonly called *cutaneous*; and in ſuch caſes it may be worth while to try the effects of medicines ever ſo partially recommended. A very ingenious foreign phyſician has lately informed me, that a chronic diſeaſe of the impetiginous claſs, which had reſiſted various other remedies, was at length completely cured by an infuſion of Ledum paluſtre taken internally. This fact did not appear ſurpriſing, when I recollected that the ſame plant had been recommended by the Swedes for the cure of Pſora. In the 8th vol. of the *Amœnitates Academicæ*, edited by Schreber, Dr. Weſtring adviſes the uſe of the infuſion as a bath. Profeſſor Pallas ſtates, that the ſame remedy is employed by the Ruſſians. A Swediſh phyſician, of the name of Wahlbom, cured a moſt obſtinate caſe of tinea by repeated waſhings with a decoction of the Ledum. Of the general aſtringent properties of the plant, I find accounts ſcattered in ſeveral writers. In Hungary it is uſed as a gargariſm in ulcerations of the throat; and it has been much extolled by Scopoli for its utility in ſuch caſes. Weſtring's Diſſertation, before mentioned, informs us, that it manifeſted a moſt decided efficacy in an epidemical angina, which was of a malignant nature; but by means of gargariſms and cataplaſms of the Ledum, yielded very ſpeedily. Jacquin records ſimilar ſucceſs attending its exhibition for diſeaſes of the throat among the inhabitants of the Carpathian mountains. Theſe evidences, however, relate principally to the external uſe of Ledum paluſtre. The exhibition of it internally is not without conſiderable teſtimony in its favour. Falk mentions, that in ſome parts of the Ruſſian empire, it is not an unfrequent remedy for intermittents. But what would lead me to form a ſtill higher idea of the value of this vegetable, is a fact recorded by the celebrated Linnæus, in his Travels to Weſtrogothia. He ſays, that the common people in that province cure Pertuſis by means of the infuſion. It was through the great naturaliſt, probably, that the medicine obtained a place in the Pharmacopeia of the Swedes, among whom, I underſtand, it is very generally uſed. Roſenſtein (in his Diſeaſes of Children) recommends it for the hooping cough; but it is alſo employed as a remedy for diarrhœa and dyſentery among his countrymen.

Theſe obſervations, to which I only regret that I have nothing to add founded on any experience of my own, may be ſufficient perhaps to induce Engliſh practitioners to make trial

of

of a plant which would ſeem to poſſeſs very active properties. Its very odour indicates an aromatic quality, though this is not ſo ſtrong in the ſpring as during the ſtate of floreſcence. Weſtring mentions a curious fact reſpecting the permanence of the aroma. A dried ſpecimen, which he found in the Burſerian Herbarium at Upſal, retained a very ſenſible fragrance, though upwards of 150 years had elapſed from the time of its being gathered. An infuſion of the leaves is ſomewhat bitter, and conveys a ſenſation to the palate ſimilar to that produced by turpentine; a good evidence of its poſſeſſing an aſtringency. There is undoubtedly a narcotic quality in a ſtrong decoction of the plant, ſome of the northern nations being accuſtomed to boil ſticks of it in beer, for the ſake of imparting to the latter an inebriating effect; and this liquor has been obſerved to occaſion violent vomitting, cephalalgia, and even ſyncope. Some caution, therefore, is neceſſary in the exhibition of the vegetable; and though in many caſes it may be adviſeable to keep up that ſtate, reſembling ſtupor, by which the advantageous effects of cicuta and other narcotics are produced, the doſe ought to be ſmall at firſt. Of the recent plant, four ounces may be infuſed in a quart of boiling water, and two ounces of the ſtrained liquor are ſufficient for the firſt exhibition.

Ledum paluſtre grows ſo abundantly in all the northern parts of the continent, that large quantities may be obtained without difficulty; though, as its favourite ſoil is of a moiſt, boggy nature, and as it is found on the declivities of lofty mountains, the cultivation of this plant may not often prove very ſucceſsful. No animals appear to be fond of it, excepting the goat, which Linnæus tells us, (in the Pan Succus) will often brouze it; and there is a ſpecies of phalæna that dwells on its leaves; but inſects in general ſhun, and are ſo much affected by its odour, that the ſmoke is uſed for the extermination of lice and other vermin. The ſpirit obtained by diſtillation would ſeem likely to be moſt efficacious for this purpoſe.

There is another ſpecies of Ledum (ſcarcely deſerving, however, to be conſidered as different) which grows very plentifully in the vicinity of Hudſon's Bay, in North America, and is generally known by the name of *Labrador Tea*, an appellation that indicates its being uſed as a beverage by the natives of that diſtrict, probably on account of this plant poſſeſſing qualities ſimilar to thoſe of Ledum paluſtre,—I mean more particularly, that of intoxicating. Ledum latifolium, as the name implies, is diſtinguiſhed by its broader leaves; but in no other reſpect does it appear to differ from its congener. I have taſted infuſions of the dried leaves, and found the flavour the ſame. This plant is well deſcribed by Jacquin in his *Collecta-*

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nea, tom. ii. and in the new edition of the *Species Plantarum*, by Wildenow, diſtinguiſhed thus, *Ledum (latifolium) foliis oblongis, margine revolutis, subtus tomentoris floribus subpentandris*, tom. ii. p. 602.

P. S. Since the above was written, I have found in the Tranſactions of the Academy of Sciences of Stockholm, (1774) an account of the efficacy of Ledum paluſtre as a remedy for Lepra. Dr. Odhelius ſpeaks of his ſucceſs in the exhibition of it with the higheſt ſatisfaction.

W. G. M.

Craven Street, July 3, 1800.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

YOU have given ample room to the effects of Digitalis, and communicated much uſeful information on the ſubject of the Cow-pox, the happieſt diſcovery of the preſent age; will you allow a little room in your valuable publication for the following caſes of the efficacy of Tractors?

Having furniſhed my ingenious friend, Mr. Biron, Houſe Apothecary to the General Infirmary here, with a pair of Tractors, he loſt no time in making the experiments; the reſult of which, I will detail in his own words.

Robt. Wood, aged 67, on June 4th was operated upon with Tractors, for a rheumatic affection of his hip, which he has had for theſe eight months. During the application of the Tractors, which was continued for ſeven minutes, no effects were produced, except a profuſe perſpiration and a general tremor. On ceaſing the application of the Tractors, to his inexpreſſible joy, and our ſatisfaction, the good effects of our labour were now produced and acknowledged, for he voluntarily aſſured us that he could walk with perfect eaſe, for he had the entire motion of the joint, and that he was free from pain. To uſe his own words, "As to the pain I have now, I do not care if I have it all my life; that will matter nothing; you may take your medicines, I'll have no more of them; theſe are the things for me." And prior to his leaving the Infirmary, he remarked how very warm thoſe parts were, where the Tractors had been applied, and then walked from the Infirmary to his own houſe, aſſuring his companion Bowſon, that he could very well walk to Beverley. June 5th, walked to the Infirmary this morning with

with very trifling difficulty; was so much pleased with the relief, or rather cure obtained yesterday, that, to use his own words again, he had very joyfully spread abroad the intelligence to his acquaintance. Has had some return of pain this morning, which however, was removed by another application; and when asked how he felt, declared, "as bonny as augh," and then marched off, with a countenance expressive of his gratitude for the wonderful relief he had obtained.

Robert Bowser, aged 37. June 4th, Pain and weakness in his right arm, which he has had for some months; after applying the Tractors one minute and a half, feels less pain, 2° heat; 4°, pain much increased; 5°, compared the Tractors to red hot needles; the application of the Tractors was continued four minutes longer; as the pain increased so did the heat to a violent degree. On moving the arm after the operation, he assured us he was very greatly relieved. 5th, His pain has lessened very considerably since yesterday, but moves his arm yet with difficulty. 6th, He returned home yesterday, and says, he slept for near three hours, when he was suddenly awoke by a violent burning heat in the arm, in such directions as the Tractors, after which he was much better.

Ann Hill, aged 57. Pain in her right arm and shoulder, which she has had for nine months; the Tractors were applied one minute and a half, when she perceived an increase of warmth on the part; 3°, the pain removed from the shoulder to the elbow; 5°, she suddenly exclaimed that she was now cured of the worst pain. "Bless me! why, who cou'd have thought it, that them little things cou'd pull the pain from one? Well, to be sure, the longer one lives the more one sees; ah dear! Well, thank God, I hope I shall be able to wash again, and earn a bit of bread; well, I can get my gown on now, in the morning I cou'd not, if it had been ever so; well, gentlemen, I return you many thanks; I reckon you'll do me again, and then you'll pull it all out." June 5th, Had pain in the shoulder once last night, but since then it has been chiefly near the elbow. The Tractors were applied as before, a greater warmth was produced in the part than before. 6th, She assures us the pain is now trifling, and complains only of weakness in the part. 8th, As she is now free from pain, wishes to return thanks.

John Smith, aged 39. Pain in the knee, ankle, and foot. June 10th, the Tractors were applied one minute and a half, when a general warmth was very evident; 2°, much less pain; 3°, the Tractors carry heat with him wherever they go. His feet prior to the application were always cold, now agreeably warm; in six minutes he assured us he could walk with greater

eaſe than he had done for three months; and to convince us how greatly he was relieved, he repeatedly, with great violence, ſtruck his feet againſt the tables and chairs. June 12th, the pain is much relieved, but the ſoreneſs in the ſoles of the feet ſtill remains; 2°, much warmth wherever the Tractors are carried; 3°, Do you think yourſelf much relieved? "Relieved! ſir, I believe I am; why, I am quite a new man from what I was." June 13, no pain in the knees or ankles, a trifling ſtiffneſs in the hip joint, which was removed by applying the Tractors round the joint for five minutes only.

Thos. Jones, aged 70. Pain in the hand and arm; with ſome difficulty he was able to move the fingers, June 13. As Wood and Smith were with this man previous to the operation, and had acquainted him with the wonderful efficacy of theſe Tractors producing warmth wherever carried, and immediately removing, or wonderfully leſſening the pain, I concluded that the ſame effects would be produced of courſe upon his arm. I determined to try if we could not produce a contrary effect; I told him I thought his caſe differed very materially from common rheumatiſm, and that a caſe of that kind was never immediately relieved by the application of the Tractors; on the contrary, that the pain was frequently increaſed for ſome hours, and that no additional warmth was perceived in the part, and that generally the patient ſlept ill the greater part of the firſt night, but that in the morning the good effects would ſoon be diſcovered. After applying the Tractors five minutes, he aſſured me that the pain was conſiderably increaſed; before he left the Infirmary, the pain was ſo violent, that he was unable to move his arm. June 14th, "You are a mere prophet, ſir; never did a poor devil ſpend ſuch a night; I toſſed and tumbled about till five o'clock, in ſuch pain, and then I got ſuch nice ſleep, and I have been eaſier ever ſince; you'll cure me; and if you do, I'll remember you, for I have a good ſhot, and they ſay there's plenty of birds." The Tractors were applied again, when the additional warmth was produced, as in the other caſes, with ſome trifling diminution of pain.

I am, GENTLEMEN, your's, &c.

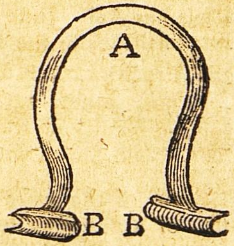
Hull, June 19, 1800. JOHN ALDERSON, M.D.

P. S. I ſhall make no comments. The Tractors were made of two pieces of wood, and covered, the one with red, and the other with black ſealing wax, and carefully kept in cotton, &c. I have ſhewn the patients to the whole faculty of the houſe, to whoſe interrogations they have anſwered as here detailed, and I have this day ſeen them return thanks at the board of truſtees, taking with them a paper for the clergy of their reſpective churches, in which they will next Sunday return their ſolemn thanks to the Almighty for their cures.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

PERMIT me to offer the model of a Speculum Oculi, for insertion in the Medical and Physical Journal. It consists of two parallel grooves, connected by a gently elastic steel arch; when we wish to apply it, the eye-lids are first opened, till they are inserted in the grooves of the Speculum; the spring is then let go, and the eye is exposed by the action of the spring, which is held in its place by the re-action of the eye-lids, without the aid of an assistant; an improvement, desirable in the operation of extracting cataract.



The anecdote which gave rise to the idea of the instrument, was as follows: A late medical gentleman in Edinburgh, performing the operation of extraction, employed the usual speculi of drawing down the under eye-lid by the hook, and elevating the upper one by the curved wire and an assistant. His assistant was not sufficiently skilful or attentive, so that when the cornea was cut through, and the extraction about to be performed, an unguarded pressure was made upon the eye with the wire, and the whole humours burst out. It is evident that this instrument will prevent the like accident in future. I have tried it in real practice, and it has completely answered my expectations, as well as my other medical friends. I remain,

GENTLEMEN,
Your most obedient,
Paisley, July 8, 1800. THOMAS RICHMOND.

A. The steel arch. B. B. The parallel grooves.

STATE OF DISEASES IN LONDON.

Account of Diseases in an Eastern District of London, from the 20th of June to the 20th of July, 1800.

	No. of Cases.		No. of Cases.
ACUTE DISEASES.		Dysentery - - - - -	3
Typhus mitior - - - -	7	CHRONIC DISEASES.	
Typhus gravior - - -	3	Phthisis Pulmonalis - - -	3
Small-Pox - - - - - -	7	Cough with Dyspnœa - -	6

Hæmoptoe	3	Psora	2
Hydrothorax	4	Tinea	2
Gastrodynia	3	Herpes Pustulosus	1
Dyspepsia	10	Chronic Rheumatism	10
Diarrhœa	16	PUERPERAL DISEASES.	
Colica Pictonum	1	Low Puerperal Fever	2
Ascites	6	Menorrhagia lochialis	3
Anasarca	4	Ischuria	1
Prolapsus Ani	1	INFANTILE DISEASES.	
Intestinal Hæmorrhagy	2	Diarrhœa	6
Cephalalgia	8	Watery Gripes	3
Vertigo	5	Aphthæ	4
Hemiplegia	3	Scrophula	2
Epilepsy	4		

One of the cases of small-pox referred to in the list, was that of a pregnant woman, upon whom the pustulary eruption appeared a few days previously to her labour. The disease was of the confluent kind, and, as might be expected, in a few days after her delivery, it terminated fatally. The child was born without any marks of infection; but on the fourth day after its birth, an eruption appeared on the face and neck, and on different parts of the body, which proceeded in the usual way to form pustules, of which there was a considerable number, and in a few days it fell a victim to the disease.

A variety of opinions has been formed respecting the effect of the variolous infection on the fœtus in utero; a sufficient number of instances, however, has been recorded, to ascertain that the disease may be communicated from the mother to the child. In some cases, the body of the child, at its birth, has been covered with pustules, and the nature of the disease has been most satisfactorily ascertained by inoculating with matter taken from the pustules.

In other cases, as in the present instance, there has been no appearance of the disease at the time of the birth; but an eruption and other symptoms of the disease have appeared so early as to ascertain that the infection must have been received previously to the removal of the child from the uterus. Thus, as Dr. Meade observes, the small-pox which the infant has contracted in the womb, breaks forth on the second, third, or any other day before the 8th from the delivery.

MONTHLY

MONTHLY REPORT of DISEASES,

Admitted under the Care of the PHYSICIANS of the FINSBURY DISPENSARY, St. John's Square, Clerkenwell.

The District, in which the Patients of the Finsbury Dispensary are visited, comprehends the Parishes of St. James, and of St John, Clerkenwell; of St. Luke; of St. Sepulchre within and without; of St. Bartholomew, the Great and the Less; the Liberties of the Rolls, and of Glass-House Yard; the Town of Islington; the Parishes of St. Pancras; of St. Andrew, Holborn; and of St. George the Martyr, Queen's-square. This Tract of Ground may properly enough be termed, a North Western District of the Metropolis.

LIST of DISEASES, &c. from June 20, to July 20.

	No. of Cases.		No. of Cases.
Mania - - - - - - - -	4	Paralysis - - - - - - -	2
Typhus - - - - - - - -	29	Cough and Dyspnœa - -	8
Psora - - - - - - - - -	5	Chronic Rheumatism - -	6
Anasarca - - - - - - - -	4	Acute Rheumatism - -	2
Dyspepsia & Hypochondriasis	21	Lumbago - - - - - -	1
Amenorrhœa - - - - -	11	Diarrhœa - - - - - -	7
Leucorrhœa - - - - - -	6	Hæmoptysis - - - - - -	3
Menorrhagia - - - - - -	3	Pneumonia - - - - - -	1
Infantile Diseases - - -	14	Asthenia - - - - - -	2
Cephalæa - - - - - -	3	Phthisis - - - - - - -	8
Dysentery - - - - - -	2	Hysteria - - - - - - -	3
Sore Throat - - - -	1	Petechiæ - - - - - - -	1
Hæmorrhois - - - - - -	1	Cynanche - - - - - - - -	4
Worms - - - - - - - -	4	Pertussis - - - - - - -	2
Schrophula - - - - -	6	Herpes - - - - - - - -	4
Jaundice - - - - - - -	6	Hydrocephalus - - - - - -	3

The heat of the present being so remarkably more intense than that of many preceding summers, has produced, as might have been expected, an evident and very important influence upon the diseases of the last month.

Typhus now prevails, attended almost uniformly with coma, and a very high degree of delirium.

After the emetic, diluents, and aperients usually employed in the first stage of this disorder, recourse was in general had to the washing of the patient with cold water,* to the use of the

* In the cases alluded to there were no conveniences for the administration of cold water, in the manner which has been recommended by Dr. Currie, of Liverpool; whose respectable authority would otherwise have been alone sufficient to have authorised the experiment. It would seem almost improper to mention the name of that distinguished writer, without expressing an admiration of his talents; and still more, of his zealous exertion of them in the advancement of medical information.

He

the Peruvian bark, and, in ſome inſtances, to a very liberal adminiſtration of wine; for which, in the caſe of this fever, perhaps, no adequate ſubſtitute is to be found amongſt all the variety of the Pharmacopeia.

The preſent virulent nature of typhus might be elucidated by a circumſtance that is perſonally intereſting to the drawer up of this report. Within the contracted ſphere of his knowledge in London, more than one medical practitioner, in attending patients afflicted with this diſeaſe, have, during the courſe of this laſt month, fallen a victim to the fatal malignity of its contagion.

One patient, a very ſhort time after the attack of the fever, was ſeized with a paroxyſm of madneſs, the violence of which ſoon put a final cloſe to his exiſtence. It ſhould be remarked, however, that in this particular caſe, a ſtrong prediſpoſition to inſanity had probably been induced by various habits of moral irregularity. When, either by a life of debauchery, or the corroding operation of any chronic paſſion, the ſtructure of the mind has been diſorganized, there is little hope from either medical or moral regimen of an entire and permanent reſtoration.

The caſe of mania noticed in the report of laſt month as combined with religious fanaticiſm, took place at one of thoſe periods of life which, in females, operate ſo frequently as exciting cauſes of this diſeaſe. Another patient, that occurred about the ſame time, became decidedly and violently maniacal, in conſequence, as there was reaſon to believe, of a very ſevere misfortune which he had experienced on the day preceding the attack of his diſorder. Both of theſe patients were ſoon reſtored to health, without any conſiderable degree of medical interference.

When mental derangement originates from either of the ſources that gave riſe to the complaint in the two inſtances laſt mentioned; that is, either from a phyſical ſtate that exiſts only for a ſhort period, or from the ſudden impreſſion of an unlooked-for calamity, an expectation of cure may not, perhaps, in many inſtances, be unreaſonably entertained.

In the caſes of dyſpepſia and hypochondriaſis, which were of very long ſtanding, the preſcriptions of the phyſicians were principally confined to country air, cold, and, if poſſible, ſea-bathing; and, amongſt other things, a reſpite from the uſe of

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He who affords inſtruction to phyſicians, muſt appear, in an eminent degree, to deſerve the general gratitude of mankind, when it is conſidered how much their health, a circumſtance ſo eſſential to the value of exiſtence, is dependant upon the ſkill of that profeſſion.

drugs.

drugs, which, when they become, as in ſuch inſtances they too frequently do, the daily food of a perſon for many years, cannot fail eſſentially to impair the organs of digeſtion, and thereby to aggravate, in the end, thoſe diſeaſes which they are intended, and might at firſt have been calculated, to relieve. A caſe may ſometimes appear to be the reſult of a very long courſe of medicine, merely becauſe a very long courſe of medicine allows time for the operation of Nature, which of itſelf will often reſtore a patient, in ſpite even of all the nauſeating doſes that are adminiſtered. By no means, however, is it intended to deduct, in the ſlighteſt degree, from the true value of pharmaceutical remedies, which are allowed to be, in many inſtances, highly important, and even abſolutely neceſſary to the cure of a vaſt variety of diſorders. A proof of their ſalutary efficacy was lately exhibited to one of the phyſicians of the Diſpenſary, in the caſe of a patient, who, affected with an obſtruction of the biliary ducts, accompanied with the countenance and complexion appropriate to jaundice, the moſt extreme dejection of ſpirits, and nearly an entire failure of all the voluntary muſcles, was, in a few days, reſtored to ſtrength, cheerfulneſs, and the phyſiognomy of health, principally by the energy of mercurial preparations.

W. W.
J. R.

Hatton Garden, July 21, 1800.

Diſeaſes admitted under the Care of the Phyſicians of the Weſtminſter Hoſpital, from the 21ſt of June, to the 23d of July, 1800.

Continued Fever	26	Dyſpepſia	3
Quotidian	1	Dyſpnœa	2
Pleuriſy	2	Dyſuria	1
Eryſipelas	1	Epilepſy	2
Meaſles	3	Gaſtrodinia	2
Chicken Pox	1	Hæmatemeſis	2
Scarlet Fever	1	Hæmorrhois	1
Amenorrhœa	5	Hooping Cough	3
Anaſarca	4	Hydrothorax	3
Angina Pectoris	1	Itch	9
Aſthenia	10	Jaundice	1
Aſthma	3	Lepra Græc	2
Catarrh	2	Leucorrhœa	3
Cephalæa	5	Menorrhagia	3
Cholera	3	Obſtipatis	2
Colic	2	Paralyſis	3
Convulſions	2	Phthiſis	5
Cough	9	Pleurodynia	1
Diarrhœa	4	Podagra	2

Rheumatiſm

Rheumatiſm	5	Syphilis	1
Struma	2	Tinea	2
Syncope	1	Worms	4

Experiments, by Dr. CHARLES DARWIN.

[Continued from p. 50—53 of our laſt.]

EXP. XV. Two ounces of viſcid *mucus*, blown from the noſe, were put into a glaſs; to theſe were added two drachms of *alkaline lixivium*; a *complete ſolution* ſoon took place; and upon the addition of two oz. of water, there did not occur any ſeparation. The glaſs was allowed to remain at reſt for the ſpace of a fortnight, and during that time a very ſlight ſeparation only had taken place; which conſiſted ſolely of light flakes of mucus ſwimming in the water; but nothing appeared in the bottom of the glaſs, which could in the leaſt degree deſerve the name of ſediment. Previous to making this and the preceding experiments, it is neceſſary to examine the water to be uſed, as I once was ſurprized to ſee a conſiderable cloud, and then a ſediment occur on adding water to mucus and alkaline lixivium; but I ſoon diſcovered that this was owing to the impurity of the water added. Alſo in conducting this experiment, we muſt let the mucus and the alkaline lixivium remain at leaſt an hour united, before we add the water; and if we obſerve any mucus undiſſolved by the alkali, it ſhould be removed before we add the water.

N. B. In ſome repetitions of theſe experiments, I have ſeen a ſlight cloud in the ſpace of twelve hours, but never immediately.

EXP. XVI. Two drachms of *purulent matter*, obtained in the opening of a large abſceſs, were put into a glaſs, and agitated with about as much of the *alkaline cauſtic* lixivium; in a ſhort time there was an intimate mixture, and the pus ſeemed to be diſſolved. But upon the addition of water a ſeparation took place, and in a few minutes a conſiderable ſediment, apparently of purulent matter, ſunk to the bottom. When allowed to remain at reſt, it continued in this condition for ſeveral days: But at the end of a fortnight, what had at firſt appeared as a ſediment floated in the ſurrounding fluid.

N. B. It will ſometimes be difficult to unite the pus and alkali.

[To be continued.]

On

On the Perspirable Fluids; *by* Dr. Mitchill.

[Concluded from p. 9—13 of our last.]

" Thus a solution of an alkaline salt, or of a soap, detaches all *acids* from the cuticle and from clothes, as likewise from all oily or other connections they may have formed. And the same lixivial and saponaceous mixtures remove all *greasy* substances, by virtue of the stronger attraction they exert toward these, than these possess for the cuticle, or the garment worn next to it. On the cleansing of clothes, there is one fact worthy of being mentioned. Sailors, on West India voyages, when destitute of soap, frequently wash their clothes in water, WITH THE AID OF MOLASSES, as a substitute for soap. They find this practice answer pretty well. Sugar is known to decompound nitrous acid very readily. Is not the purification of mariners' clothes, in this manner, a circumstance of weight, to evince the decomposition of the septic acid they contain, by the saccharine ingredient of the molasses?

" Such is the detail of the principal constituent parts of that nastiness which accumulates on the skins and in the garments of human beings; and such the theory of the operation of alkalies &c. in making them clean. From a comparison of the facts relative to *perspiration* with those which respect *urine*, it will readily appear, that the two fluids very much resemble each other, as might naturally be supposed from the great connection there has been long known to exist between the two discharges. What is, therefore, true of the one, is, to most purposes, applicable to the other. In particular, the sweat as well as the urine contains *septon*, which often combines with oxygen, and forms that noxious compound, *septic acid.*

" Yet it must be remembered, that as extractive matter and acids are capable of solution in mere water, and require not the aid of alkalies to detach them, that washing the cuticle in cool, or, more readily, in warm water, removes every thing but the grease, and even loosens or liquefies a part of that. Water cleanses the cuticle or a shirt by exerting a greater attraction for the foul material sticking to them, than the skin or a garment does. For this purpose the water must be clean, or, at least, less tainted than the subject intended to be purified. Upon the same principle that *water* or *alkaline ley* cleanses the cuticle, a *clean shirt* will do the same; that is, by attracting or absorbing from it something foul or foreign. *Clean shirts, drawers,* and *stockings*, are, therefore, substitutes for *clean water:* and he who changes his linen very frequently, deterges the surface of the body *almost* as much as by bathing; and, per-

haps, for the purposes of health, *quite* as much. This appears to be more certainly the case, because, after washing in soap-suds, a portion of alkali adheres to the cloth, unless it be well rinsed, and is instantly ready to exert its neutralizing effect. A dry shirt, thus washed, may be conceived as besprinkled with innumerable minute crystals of potash and soda.

" Pathologists have observed that *acid sweats* have sometimes occurred. In these cases an acescent disposition, or acid acrimony present in the blood, at the critical period of fevers, and at other times, may tincture the fluids secreted in the vessels of the skin with some of their peculiar qualities. This has been alleged to have taken place to so high a degree as to turn *blue* litmus-paper to a *red*. It is not material to my argument whether this happens frequently or never. It is enough for my purpose that the liquids excreted on the cuticle have a tendency to become acid, *after* being deposited there. If *acid secretions* ever happen, they exhibit a stronger case, and illustrate the doctrine with additional force.

" II. Another general property of these vitiated or corrupted animal excretions is to rot the cloathing to which they adhere, and cause it to wear out the faster A shirt, indeed, worn until its threads are fully impregnated with foulness, requires so much soaking and friction, while in the wash, that there can be no wonder if, in the operations of being made clean, it is almost wrung or rubbed to pieces. But, independent of all this, it is well known that linen, and cloathing of all kinds, become very remarkably weaker by lying and remaining long in their dirt, without being ever handled by the laundress. There may be such a collection and concentration of animal matter, as not only to rot the cloth, but to convert it to an infectious fomes, and make it diffuse pestilential vapours around. Garments, too long worn, without being cleansed from time to time, often grow *rotten* and *infectious* as they grow *nasty*. Alkalies remove the nastiness, and, if applied discreetly, in season, prevent the rottenness, and check the infection. Every day's experience demonstrates their efficacy in these processes. The following fact may serve as an exemplification of a very common occurrence: A gentleman, in the time of war, was taken captive, and compelled, by the enemy, to march on foot a great distance, at a violent rate, during very hot weather. He sweated most profusely. At the end of the march he was confined in prison, and treated with great rigour. Being deprived of changes of raiment, and of the means of keeping himself clean, his shirt, in a short time after his confinement, became rotten, and he fell sick of a fever. In this instance, no doubt, both the cuticle and shirt became impregna-

ted

ted with *ſeptic poiſon*, manufactured from the *ſweat*, from which both the *conſtitution* and the *clothing* were the ſufferers. In this way the American and Britiſh ſailors, who viſit the Weſt-Indies, *catch yellow fevers*, as they are called, without once going on ſhore, or having any communication with infected perſons. Numberleſs facts prove that, even in the harbours, theſe fatal diſtempers are not *introduced from on ſhore, but bred and ſupported on board the veſſels themſelves*, in which the people fall ſick, whence they are carried on ſhore to be cured.

"III. A third general effect of theſe ſeptic productions, as was obſerved in the preceding paragraph, is *to injure the conſtitution*. This they do in two ways: 1. By a direct ſtimulant operation upon the part of the *true ſkin* to which they are applied, cauſing different forms of tinea, herpes, pſora, elephantiaſis, petechial and miliary eruptions, &c. &c. and, 2. By a direct ſtimulant operation upon the heart and ſanguiferous veſſels, after they are conveyed thither through the abſorbents ariſing from the cuticular ſurface, cauſing various grades of febrile diſturbance, of the intermittent, remittent, and continued forms, and inducing diſtempered motions in the lymphatic ſyſtem, as they paſs through its branches. All examiners of this ſubject ſhould, however, recollect, that ſeptic fluids, though they exiſt on the cuticle and in the blood-veſſels, do not always ſtir up diſeaſes; for the conſtitution may be, and frequently is, in ſuch a ſtate as to reſiſt their tendency to produce morbid action; the reaſon of which ſeems to be, that the ſtimulus no longer irritates a conſtitution which is ſeaſoned or habituated to it.

"IV. A fourth general property of theſe fluids, engendered in corruption, is to be volatilized by the heat of ſummer and autumn in the United States. The noxious quality of the vapours proceeding from putrefying collections of common animal matter is too well known to require mentioning in this place. When that other animal matter which conſtitutes perſpiration and ſweat, and accumulates in the cuticle and clothing to large amount, has undergone the changes to which it is prone, it is much diſpoſed to aſſume the aërial form. Some portions of theſe exhaling vapours are often pungent, active, and of a deleterious quality. They are produced, generally ſpeaking, not in the circulating blood, nor during the proceſs of ſecretion, but *after their* depoſition upon the cuticle and clothes, and aided by HEAT and MOISTURE, through a proceſs ſimilar to that which converts urine, and ſeveral other excrementitious ſubſtances, into offenſive and noxious airs.

"Clothing, or merchandize of any kind, wherein noxious airs or fluids of the ſeptic kind are generated or accumulated, is

is called a FOMES. In this way bills of paper-money passing frequently from man to man, and becoming tainted with every thing that human hands can impart to them, have, in the city of New-York, degenerated to a *fomes.* So the shoes and stockings of that unclean person who suffers septic acid so to accumulate about the toes and feet as to eat away the cuticle to the quick, may become a *fomes.* In like manner a seaman's chest of dirty clothes, tainted with all kinds of animal exudations, may, during a voyage from Cape-Françoise to New-York, turn to a *fomes*, though not a particle of infection was borrowed from the atmosphere of St. Domingo, or from any person living or dying in it. By a process in no wise different, do feather beds, and beds of all kinds, that have been long laid upon, and thereby become thoroughly impregnated with perspirable and other matter, acquire their unwholesome qualities, and turn to a *fomes.* Just thus are clothes charged with perspirable and other excrementitious matter, discharged from the bodies of prisoners in jail, and of other persons living in foul and confined places, converted to venom and pestilence, or to an infectious or contagious *fomes.* And it is not impossible that a bale of cotton, or any similar material, may, by impregnation with animal matter, become a repository of mischief, and a *fomes.*

" In the preceding paragraph I have introduced the word " fomes" as often as I conveniently could; and have done so that I might shew the impropriety of the term, and offer my objections to it. The *literal* meaning of *fomes*, in Latin, refers to that quality, in bodies, which makes them catch and retain fire very readily; such as punk, tinder, lint-stock, and touch wood (rapuitque in *fomite* flammam—Virg. Æneid i. v. 180; in the note to which, in the Dauphin's edition, may be seen a blunder of the critics.) ACHATES used a *fomes* when he struck fire with flint, &c. *Metaphorically*, it denotes an incentive or motive to any action. While the belief prevailed, that infection was carried from place to place, like a fire-brand, and passed from person to person, like a spark, the word " fomes" was borrowed to express, by a strong and illustrative figure, the analogy between the supposed effects of fire and those of pestilence. As each of the substances just named was a fomes of fire, so a chest of clothes, a blanket, a bed, or a bale of merchandise, was a fomes of pestilence. This mode of expression was thought to be highly philosophical and correct; insomuch, that when any one affirmed that an atom of contagious matter, received into a blanket, multiplied itself as a spark of fire enlarges in a tinder box, he was thought to express himself sensibly.

" But

"But it will appear, to any one who comprehends the preceding observations, that the simile, if it be intended for a simile, is imperfect; or the metaphor, if it is meant for a metaphor, is deceitful: for it has been shewn, that the thing called a *fomes* produces pestilential matter without having a particle of infection (like a spark of fire, or even a bit of leaven) introduced into it from without; and it follows, of course, that all figurative expressions, calculated to uphold the idea *of foreign introduction* of common pestilential matter, and its *derivation ab extrà*, are highly conducive to the continuance and propagation of error. It is high time this form of speech was discontinued. This figurative expression itself, and the unhappy association of ideas which springs from it, are the cause of a large proportion of the wrong-headedness now prevalent, concerning the distempers called infectious.

"Was not the malignant fever, or plague, which destroyed a great proportion of one thousand nine hundred and forty-six convicts, on board the hulks in the Thames, (Colquhoun's Police of the Metropolis, chap. xii. p. 307) generated in the vessels? And were there not local causes enough to excite it, without the importation and introduction of a fomes? Did not the pestilential distemper which afflicted the Spanish captives at Winchester arise within the walls of their prison, independent of any fomes introduced from without? Was not Hosier's squadron, during the unfortunate expedition against Porto-Bello, dismally disabled by a distemper, bred on board and within the ships, wholly unaided by any thing like a fomes? And, if it was not superfluous, I would ask, whether malignant distempers did not harrass, with their visitations, almost all the great cities of Europe, and, indeed, of the world, until, by enforcing sufficient laws against nuisances, their inhabitants removed the causes?

"In all these instances, dreadful as some of them were, the causes of the distempers were engendered or manufactured on the spot. These exciting causes were, in all, exactly similar to that which dirties a shirt, or the commonest article of clothing. If there is any difference of moment, it is nearly this: in one case pestilence is manufactured upon a *great* scale, and, in the other, upon a *small* one. The same principle pervades both cases. The laws of Nature operate uniformly in both: *and the same means that will cleanse a shirt, will purify a ship or a jail.* Substances that subdue acids, such as OILS, CALCAREOUS EARTH, AND SOLUTIONS OF ALKALINE SALTS, CAN ACCOMPLISH THE WORK.

"And if I interpret right that accurate mineralogist FERBER, (Travels through Italy, letter xi. p. 129, edit. by Raspe) the

the catacombs, near the cities of Naples and Rome, are cut through and into masses of *tufo-stone.* This seems to consist chiefly of volcanic ashes, hardened into a substance of sufficient compactness, in many cases, for building: and those afforded by Vesuvius, whether grey, white, brown, or yellowish, contain *alkaline* or *calcareous* particles enough to effervesce with acids. Hence the preservation of human bodies in those subterranean chambers can be easily accounted for. The lime preserved these as the soda preserved the mummies. SPALLANZANI has accounted for the presence of this alkaline matter. In his very interesting travels, (Viaggi alle due Sicilie, &c. Pavia, 1792) he affirms, that a large tract of the Appenines, and the substratum of the volcanic country about Naples, consist of the *carbonate of lime.* (tom. i. p. 109—114) And as he has shewn that sundry volcanic productions, particularly the vitrified lavas of the Lipari Islands, contain *muriatic acid,* (tom. iii. p. 351) which, he thinks, is derived from the decomposition of *common salt,* (p. 361) *veramente del mare istesso penetrante per di sotto i monti volcanici,* penetrating from the sea itself, beneath volcanic mountains, does it not follow, that its *soda* must be combined wiih other fossils, and, perhaps, with the tufo?

"There has been a number of tolerable conjectures on the precise nature of pestilential matter. Besides those I have mentioned in other essays, I refer you now to the *Loimologia* of HODGES, a physician who resided in the city of London during all the time of the plague of 1665. Observe how well he has guessed. In his § 1, on the Cause of a Pestilence and a Contagion, he considers it as "arising chiefly from a change or corruption of the *nitrous spirit* in the air." (p. 33 and 37) The *nitrous spirit* of this writer, as well as of his countryman and predecessor MAYOW, was evidently *vital or oxygenous air*; and, by its change or corruption, may be obviously understood, a combination of it with septon, the principal of putrefaction, or azote, the element hostile to life. To this change in the nitrous principle, Hodges ascribes "blasts upon trees and diseases among cattle," as well as a pestilence among mankind, (p. 41); and to somewhat of *an acid,* sometimes produced in this way, he attributes the quality of rain-water to stain the clothing and skins of travellers in the equatorial regions of Africa, and "burn upon them, as it were pestilential characters." (p. 42) He affirms, too, what I have often witnessed, that there is a great similitude between a pestilence and a scorbutic habit, (p. 46); and declares, (p 49) that the convulsions and vellications of the stomach and alimentary canal are not owing to a putrefaction

of

of bile, nor an eruption of fire, as the manner of thinking then was, but to this " vitiated ſaline ſpirit." This author makes favourable mention (p. 171) of ſpirit of hartſhorn, (ammoniac) given in doſes of two ſcruples and one drachm.

" I ſhall, however, reſt the argument here for the preſent. If the reaſoning advanced eſtabliſhes the principle, then the principle ſo eſtabliſhed may be employed to explain innumerable other facts in Nature belonging to this head. Your ſagacity will judge both of the rule and its application.

" Be aſſured I am yours,

" With high conſideration and reſpect,

" SAMUEL L. MITCHILL."

On the GALVANIC ELECTRICITY.

SINCE the account of the extenſion of Galvani's experiments, given at one of Sir Joſeph Banks's meetings, and ſoon after by Dr. Garnet at the Royal Inſtitution, we have been very deſirous of preſenting our readers with an accurate ſtatement of what is now known on that curious and intereſting ſubject. Mr. Nicholſon's Philoſophical Journal for July, furniſhes us with the moſt authentic means.

The 9th article of that number is intitled, " *Account of the new Electrical or Galvanic Apparatus of Sig.* ALEX. VOLTA, *and Experiments performed with the ſame.*

" From motives of delicacy," ſays Mr. Nicholſon, " to the inventor of the moſt curious and important combination hereafter to be deſcribed, I forbore giving an account of its conſtruction and effects in the laſt number of this Journal, though it has now been a ſubject of great attention among philoſophers for near two months. It appeared proper to avoid the publication of facts, originally flowing from the liberal communication of the worthy preſident of the Royal Society, until the paper of the inventor had been read to that learned body; and this could not be done till very lately, becauſe the latter part of his memoir did not arrive till long after the firſt four pages.

" The Right Honourable Sir Joſeph Banks, Bart. P. R. S. having favoured my friend Anthony Carliſle, Eſq. with the peruſal and conſideration of theſe four pages at the latter end of laſt April, I had the pleaſure to look them over with him, immediately after which he conſtructed an inſtrument according to Sig. Volta's directions. The experiments made with this

this will form part of the present communication; but in the first place, I shall endeavour to relate the leading particulars of the communication made to the Royal Society, which no doubt will hereafter appear at large in their Transactions.

" The portion of letter which first arrived from Sig. Volta, is dated from Como in the Milanese, March 20, 1800. This, together with the subsequent parts, contains a detailed account of the instrument, of which the following is one of the most convenient forms.

" Take any number of plates of copper, or, which is better, of silver, and an equal number of tin, or which is much better, zinc, and a like number of discs, or pieces of card, or leather, or cloth, or any porous substance capable of retaining moisture. Let these last be soaked in pure water, or which is better, salt and water, or alkaline lees. The silver or copper may be pieces of money. Build up a pile of these pieces; namely, a piece of silver, a piece of zinc, and a piece of wet card: then another piece of silver, a piece of zinc, and a piece of wet card: and so forth, in the same order (or any other order, provided the pieces succeed each other in their turn) till the whole number intended to be made use of is builded up. The instrument is then completed.

" In this state it will afford a perpetual current of electricity, through any conductor communicating between its upper and lower plates; and if this conductor be an animal, it will receive an electrical shock as often as the touch is made, by which the circuit is completed. Thus, if one hand be applied to the lower plate, and the other to the upper, the operator will receive a shock, and that as often as he pleases to lift his finger and put it down again.

" This shock resembles the weak charge of a battery of immense surface, and its intensity is so low, that it cannot make its way through the dry skin. It is, therefore, necessary that a large surface of each hand should be well wetted, and a piece of metal be grasped in each, in order to make the touch, or else that the two extremities of the pile should communicate with separate vessels of water, in which the hands may be plunged.

" The commotion is stronger the more numerous the pieces. Twenty pieces will give a shock in the arms, if the above precautions be attended to. One hundred pieces may be felt to the shoulders. The current of electricity acts on the animal system while the circuit is complete, as well as during the instant of commotion, and the action is abominably painful at any place where the skin is broken.

" That the energy of the apparatus is the effect of an electric

tric ſtream or current, is proved by the condenſer with which Sig. V. aſcertained the kind of the electricity and obtained its ſpark. He finds the action ſtrongeſt, or moſt pungent, on wounds on the minus ſide of the apparatus, or where the wounds give out electricity, a fact alſo obſervable in the common electric ſpark.

" The theory of the learned inventor, if I rightly apprehend him, is, that it is a property of ſuch bodies as differ in their power of conducting electricity, that when they are brought into contact they will occaſion a ſtream of the electric matter. So that if zinc and ſilver be made to communicate immediately by contact, there will be a place of good conducting energy; and if they be made to communicate mediately by means of water, there will be a place of inferior conducting energy: and wherever this happens there will be a ſtream or current produced in the general ſtock of electricity. This is not deduced as the conſequence of other more ſimple facts; but is laid down as a general or ſimple principle grounded on the phenomena.

" As the current of electricity will be reſiſted by the different conductors, he remarks that the metals may touch in a ſingle point, or be ſoldered together; but that the humid ſurfaces muſt be more extended.

" By many experiments, he finds that the conſequences are the ſame, whether the zinc and ſilver touch each other, or whether the communication be made by ſeveral different metals, provided the water be in contact with the zinc and the ſilver only.

" Where zinc is uſed, ſalt water is preferable to alkaline lees, but the contrary when tin is made uſe of inſtead of the zinc.

" The effect is much increaſed by elevation of temperature.

" He was ſurpriſed to find that the Galvanic flaſh of light was no greater with this apparatus than with a pair of plates; but is was produced when the conductor of the circuit was applied to any part of the face, or even to the breaſt. The ſtrongeſt action was when the touching plate was held between the teeth, ſo as to lie upon the tongue. In this caſe the lips and tongue were convulſed, the flaſh appeared before the eyes, and the taſte was perceived in the mouth.

" Two blunt probes were inſerted in the ears, and the ſhock paſſed through the head, after which the communication was kept up. A peculiar ſound, like crackling or boiling, was heard; but the author did not think it prudent to make this experiment repeatedly.

" The ſenſe of ſmell could not be excited, becauſe, as Sig. V. remarks, this electricity cannot be made to diffuſe itſelf in the air.

" As the diſcs become dry, and loſe their power, Sig. V. endeavoured to prevent this effect by incloſing the column in wax or pitch, and in this he has ſo far ſucceeded, that he has fitted up two colums of twenty pieces each, which have acted well for ſome weeks, and he hopes will for months.

" The combination, which he thinks the moſt inſtructive, conſiſts of a row of glaſſes or cups (not of metal) containing warm water or brine. Into each of theſe is plunged a plate of zinc and another of ſilver, not touching each other. From theſe plates reſpectively proceed tails or prolongations, which communicate with or touch the plates of the outer glaſſes in ſuch a manner, that the zinc of the firſt cup communicates with the ſilver of the ſecond; the zinc of the ſecond with the ſilver of the third; the zinc of the third, &c. progreſſively and regularly through the whole row. The communication between the firſt and laſt glaſſes gives the ſhock, &c. The plates in the fluid are directed to be about an inch ſquare; but the contacts above the water may be as ſmall as the operator pleaſes.

" Sig. Volta makes honourable mention of my conjectural theory of the torpedo. After remarking that my inductions were the moſt probable that the exiſting theory of electricity could at that time afford, he proceeds to make various objections, needleſs to be here detailed, and then offers his own new and ſtriking apparatus as more nearly reſembling the torpedinal organ. I need not anticipate the reader in the happy points of reſemblance between their ſtructure and effects.

" Thus far," ſays Mr. Nicholſon, " I have followed this able philoſopher; who, to his former reſearches into the nature and laws of electricity, has now added a diſcovery which muſt for ever remove the doubt whether Galvaniſm be an electrical phenomenon. But I cannot here look back without ſome ſurprize, and obſerve that the chemical phenomena of Galvaniſm, which had been ſo much inſiſted on by Fabbroni, more eſpecially the rapid oxydation of the zinc, ſhould conſtitute no part of his numerous obſervations.

" On the 30th of April, Mr. Carliſle had provided a pile conſiſting of ſeventeen half crowns, with a like number of pieces of zinc, and of paſteboard, ſoaked in ſalt water. Theſe were arranged in the order of ſilver, zinc card, &c. which order I ſhall denote by ſaying, that the ſilver was undermoſt, that is to ſay, under the zinc; and I make this remark becauſe ſome philoſophers have uſed the expreſſion that the ſilver was

undermoſt

undermoſt when they uſed the order of ſilver, card zinc, &c. which, as the reader will eaſily perceive, is contrary to the order here ſpoken of. This is of no conſequence to the effect, though it is material to a clear underſtanding of the terms we uſe. This pile gave us the ſhock as before deſcribed, and a very acute ſenſation wherever the ſkin was broken. Our firſt reſearch was directed to aſcertain that the ſhock we felt was really an electrical phenomenon. For this purpoſe the pile was placed upon Bennett's gold leaf electrometer, and a wire was then made to communicate from the top of the pile to the metallic ſtand or foot of the inſtrument. So that the circuit of the ſhock would have been through the leaves, if they had diverged. But no ſigns of electricity appeared. Recourſe was then had to the revolving doubler, deſcribed at page 95 of our preſent volume. The plate A was connected with the top of the electrometer and the ſilver end of the pile; and the plate B and ball were made to touch the top of the ſyſtem by an uninſulated braſs wire. The doubler had been previouſly cleared of electricity by twenty turns in connection with the earth. The negative divergence was produced in the electrometer. Repeated experiments of this kind ſhewed that the ſilver end was in the minus, and the zinc end in the plus ſtate.

" In all theſe experiments it was obſerved, that the action of the inſtrument was freely tranſmitted through the uſual conductors of electricity, but ſtopped by glaſs and other non-conductors. Very early in this courſe, the contacts being made ſure by placing a drop of water upon the upper plate, Mr. Carliſle obſerved a diſengagement of gas round the touching wire. This gas, though very minute in quantity, evidently ſeemed to me to have the ſmell afforded by hydrogen when the wire of communication was ſteel. This, with ſome other facts, led me to propoſe to break the circuit by the ſubſtitution of a tube of water between two wires. On the 2d of May we, therefore, inſerted a braſs wire through each of two corks inſerted in a glaſs tube of half an inch internal diameter. The tube was filled with New River water, and the diſtance between the points of the wires in the water was one inch and three quarters. This compound diſcharger was applied ſo that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-ſix half crowns with the correſpondent pieces of zinc and paſteboard. A fine ſtream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the ſilver, and the oppoſite point of the upper wire became tarniſhed, firſt deep orange, and then black. On reverſing the tube, the gas came from the other point, which was now loweſt,

eſt, while the upper in its turn became tarniſhed and black. Reverſing the tube again, the phenomena again changed their order. In this ſtate the whole was left for two hours and a half. The upper wire gradually emitted whitiſh filmy clouds, which, towards the end of the proceſs, became of a pea green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered ſemiopaque by what fell off, and in a great part lay, of a pale green, on the lower ſurface of the tube, which, in this diſpoſition of the apparatus, was inclined about forty degrees to the horizon. The lower wire of three quarters of an inch long, conſtantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time the emiſſion of gas was ſuſpended. When this laſt mentioned wire was removed, the gas re-appeared as before, not inſtantly, but after the lapſe of four beats of a half ſecond clock ſtanding in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread.

"It might ſeem almoſt unneceſſary to have reverſed the order of the pile in building up, as reverſing the tube muſt have anſwered exactly the ſame purpoſe. We choſe, however, to do this, and found that when the zinc was at the bottom, its effects were reverſed, that is to ſay, the gas ſtill came from the wire communicating with the ſilver, &c.

"We had been led by our reaſoning on the firſt appearance of hydrogen to expect a decompoſition of the water; but it was with no little ſurprize that we found the hydrogen extricated at the contact with one wire, while the oxygen fixed itſelf in combination with the other wire at the diſtance of almoſt two inches. This new fact ſtill remains to be explained, and ſeems to point at ſome general law of the agency of electricity in chemical operations. As the diſtance between the wires formed a ſtriking feature in this reſult, it became deſirable to aſcertain whether it would take place to greater diſtances. When a tube three-quarters of an inch in diameter, and thirty-ſix inches long, was made uſe of, the effect failed, though the very ſame wires, inſerted into a ſhorter tube, operated very briſkly. The ſolicitation of other objects of enquiry prevented trial being made of all the various intermediate diſtances; but from the general tenor of experiments, it appears to be eſtabliſhed, that this decompoſition is more effectual the leſs the diſtance between the wires, but that it ceaſes altogether when the wires come into contact.

"May 6.—Mr. Carliſle repeated the experiment with copper

per wires and tincture of litmus. The oxydating wire, namely, from the zinc side, was the lowest in the tube; it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it seems either an acid was formed, or that a portion of the oxygen combined with the litmus, so as to produce the effect of an acid.

" It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or scarcely three; that from a series of glasses set up by Mr. Carlisle, as well as from the pile itself, it appears that the same process of decomposition of water is carried on between each pair of plates, the zinc being oxyded on the wet face, and hydrogen given out; that the common salt is decomposed, and exhibits an efflorescence of soda round the edges of the pile, extruded, most probably, by the hydrogen: and that on account of the corrosion of the faces of the zinc, it is necessary to renew them previous to each construction of the pile. This may be done by scraping or grinding. I found it most convenient to lay the piece in a hole in a board, and give it a stroke with a float file, or file of which the teeth are not crossed. It might, perhaps, be less troublesome to clean them with diluted muriatic acid; but this I have not tried.

" As the ample field of physiological research to which Mr. Carlisle's attention is directed, and the multiplicity of my own avocations, rendered it less convenient for us to pursue our enquiries together, I constructed an apparatus for my own use. Zinc was laminated to the twenty-fourth part of an inch in thickness, and pure silver to the one-thousandth part of an inch, that is to say, as thin as our flatting mills can bring it.

" Of these metals I made two sets, namely, sixteen pieces of silver of two inches in diameter, and sixteen pieces of 1.8 inch diameter, with their correspondent plates of zinc and wetted card. The small pile was first prepared, and whether it were that these thin pieces were more disposed to admit the water between the metallic faces of contact, or from whatever other cause it may have arisen, it did not appear by any experiment, that the whole set, though so greatly exceeding the pile of half-crowns in surface, was capable of doing more in the decomposition of water, or in communicating the shock. But this, with other facts, seems to shew, that the repetition of the series is of more consequence to this action, than the enlargement of surface; and also that the thickness of the plates, though it may be attended with convenience, most probably affords no addition to the force. I must also add, that I have no reason to recommend my pile, though at first sight it seemed

to

to possess cheapness and convenience. The plates of zinc are too thin to bear frequent cleaning or renewing after corrosion of the surface, and the silver, though it is scarcely acted on in this situation, is too thin to be conveniently wiped or handled.

"The spontaneous electricity of the doubler presented an objection to the strict fidelity of its results; whence I thought it desirable to give my pile a trial with the condenser. The foot or stand of my electrometer is a brass plate truly flat, and 3.8 inches in diameter. A piece of thin Persian silk was tied smoothly upon the face of this plate, and it was then placed upon another brass plate, upon which it was moved about horizontally, in order to accumulate electricity by friction; the electrometer itself being used as the handle by grasping the top. It was found that this treatment produced very weak signs of electricity when the electrometer was lifted up. The lower brass plate was then placed on the top of the small pile, and the condensing electrometer placed upon it. A communication was then made, by means of a wire from the lower or silver end of the pile to the upper plate of the condenser, or foot of the electrometer. In this situation it is evident, that the charge of the pile was employed in producing opposite states of electricity in the condensor, which would be shewn when the plates came to be separated. The wire of communication being taken away, the electrometer was lifted, and the leaves diverged and struck. It became necessary, therefore, to repeat the experiment, taking care to lift the electrometer more gradually. The divergence took place as before, and it was increased by presenting excited sealing wax towards the bottom of the electrometer. And as the top of the pile had by compensation diminished the same divergence, it is clear that the electricity of the top of the pile, *viz.* of the zinc, was contrary to that of sealing wax; that is to say, the zinc was in the plus state. After a number of repetitions of this experiment with the same invariable result, the pile was then carefully overset, without disturbing the relative arrangement of its parts; so that the zinc was now at the bottom, and the silver at the top. The electricity of the silver was then tried a number of times, by precisely the same process as before, and it exhibited an equal degree of intensity, but it was minus or negative. In one of these experiments, I certainly saw the spark at the time of completing the circuit, and afterwards with the same pile, when I was expressly looking for it. But it is less necessary to dwell on these facts, as the stronger combinations have exhibited this effect with much greater perspicuity.

"The decomposition of water, and oxydation of metallic wire,

wire, gave birth to a variety of ſpeculations and projects of experiments. Among others it became a queſtion, what would be the habitude of metals of difficult oxydation. Two wires of platina, one of which was round, and one-fortieth of an inch in diameter, and the other nearly of the ſame maſs, but flatted to the breadth of one twenty-fifth of an inch, were inſerted into a ſhort tube of a quarter of an inch inſide diameter. When placed in the circuit, the ſilver ſide gave a plentiful ſtream of fine bubbles, and the zinc ſide alſo a ſtream leſs plentiful. No turbidneſs nor oxydation, nor tarniſh appeared, during the courſe of four hours continuance of this operation. It was natural to conjecture, that the larger ſtream from the ſilver ſide was hydrogen, and the ſmaller oxygen. Thick gold leaf was tried with the ſame effects. A wire of braſs was then ſubſtituted inſtead of one of the ſlips of gold. When the braſs was on the minus, or ſilver ſide, the two gaſes were extricated for two hours, without oxydation as before; but when the braſs was, by reverſing the tube, brought to the plus ſide, it became oxyded in the ſame manner as if both the wires had been braſs. When the ſlips of gold were long ſubjected to this action, the extremity of the ſlip communicating with the zinc, acquired a coppery or purpleiſh tinge, which was deepeſt near the end. Whether this aroſe from oxydation of the gold, or of the copper, of which gold leaf contains about a ſeventieth part, cannot from this experiment be decided.

" The ſimple decompoſition of water by platina wires without oxydation, offered a means of obtaining the gaſes ſeparate from each other. With this intention, Mr. Carliſle's pile of thirty-ſix was combined with my two ſets of ſixteen repetitions. His pile was built with the zinc uppermoſt, and mine in the reverſe order; ſo that by connecting the upper plates the whole conſtituted one range, and the communications could be made from the bottom of the one to the bottom of the other. The two platina wires were made to protrude out of two ſeparate tubes, each containing a little water, and through the oppoſite corks of each were paſſed copper wires of communication. Theſe tubes were ſlightly greaſed on the outſide to prevent their becoming damp; and in this ſtate the extremities, armed with the platina, were plunged in a ſhallow glaſs veſſel of water, in which two ſmall inverted veſſels, quite full of water, were ſo diſpoſed, that the platina of one tube was beneath one veſſel, and the platina of the other tube was beneath the other, the diſtance between their extremities being about two inches. The copper wires of theſe tubes reſpectively were made to communicate with the extremities of the intire pile of ſixty-eight ſets. A cloud of gas aroſe from each wire, but

but moſt from the ſilver, or minus ſide. Bubbles were extricated from all parts of the water, and adhered to the whole internal ſurface of the veſſels. The proceſs was continued for thirteen hours, after which the wires were diſengaged, and the gaſes decanted into ſeparate bottles. On meaſuring the quantities, which was done by weighing the bottles, it was found, that the quantities of water diſplaced by the gaſes, were reſpectively, 72 grains by the gas from the zinc ſide, and 142 grains by the gas from the ſilver ſide; ſo that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter. Theſe are nearly the proportions in bulk of what are ſtated to be the component parts of water. The gas from the zinc ſide, being tried with one meaſure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another meaſure; the gas from the ſilver ſide, by the ſame treatment, contracted to 1.6. The air of the room, on trial, contracted to 1.28. From the ſmallneſs of the quantity no attempt was made to detonate the air from the zinc ſide, but a portion of that from the ſilver ſide being mixed with one-third of atmoſpheric air, gave a loud detonation.

"Upon the above it may be remarked, that it does not ſeem probable that oxygen was afforded by both wires, but that they were mixed by the circumſtances of the experiment. For the gaſes being extricated in extremely minute bubbles beneath the inverted veſſels, cauſed a ſlow aſcending current conſiſting of water mixed with thoſe bubbles, many of which were undoubtedly too ſmall to be diſcerned. This aſcending current gave out as much of its gas at the top of the veſſel, as had time to conglomerate; but the extremely minute bubbles would return in the deſcending current, and be repeatedly carried up before this effect could take place. Such a continual circulation, or ſtream, the lower part of which paſſed down into the ſaucer, muſt at length have occaſioned the whole maſs of fluid to become replete with theſe minute bubbles, which would break at the open ſurface, and be loſt, or attach themſelves to the ſides of the veſſel, as was ſeen to be the caſe. What proportion may have thus diſappeared is uncertain; but it is highly probable, that one conſequence of the imperfections of our apparatus, was to occaſion both the inverted veſſels to become receptacles for the gaſes from both wires indiſcriminately; though moſt plentifully in each, from the wire immediately beneath its mouth. If this reaſoning may be admitted for the preſent, till the experiment is repeated in cloſed veſſels, it will be fair to reckon the whole diminution on both the quantities. The whole diminution was 1.15, whence it would follow,

follow, that the purity of the oxygen estimated in Priestley's manner, would be expressed by the number 0.85.

"On account of the length of this communication, I shall at present forbear to enter into any considerations of theory, but shall conclude with a concise mention of the effects of a pile of one hundred half crowns, and a chemical incident, which appears to be the most remarkable of those which I have yet observed.

"The pile was set up with pieces of green woollen cloth soaked in salt water. It gave severe shocks, which were felt as high as the shoulders. The transition was much less forcible through a number of persons, but it was very perceptible through nine. The spark was frequently visible when the discharge was made in the dark, and a gleam of light was also, in some instances, seen about the middle of the column at the instant of the explosion. The assistants were of opinion that they heard the spark.

"The extrication of the gases was rapid and plentiful by means of this apparatus. When copper wires were used for the broken circuit, with muriatic acid diluted with 100 parts of water in the tube, no gas, nor the least circulation of the fluid was perceived, when the distance of the wires was two inches. A short tube, with two copper wires very near each other in common water, was made part of the circuit, and shewed by the usual phænomena, that the stream of electricity was rapidly passing. The wires in the muriatic acid were then slided within a third of an inch of each other. For the sake of brevity, I avoid enumerating the effects which took place during several hours, and simply state; that the minus wire gave out some hydrogen during an hour, while the plus wire was corroded, and exhibited no oxyde; but a deposition of copper was formed round the minus, or lower wire, which began at its lower end: that no gas whatever appeared in this tube during two hours, though the deposition was going on, and the small tube shewed the continuance of the electric stream; and that the deposition at the end of four hours formed a ramified metallic vegetation, nine or ten times the bulk of the wire it surrounded.

"In this experiment it appeared, that the influence of electricity increasing the oxydability of the upper wire, and affording nascent hydrogen from the lower, caused the latter to act as the precipitant of a solution of one and the same metal.

"We are in want of a measure of the intensity of the action of these machines. Will this be derived from the quantities of water decomposed, or of gas extricated under like circumstances in given times? Or from any change of temperature?

perature? Or what other commenſurate incident?—Mr. Carliſle has not found that the water in the tube, while under this agency, did produce the ſlighteſt effect on a very ſmall and delicate thermometer."

The next article contains, "*Some Experiments and Obſervations on Galvanic Electricity. By Mr.* W. CRUICKSHANK, *Woolwich. Communicated by the Author.*

"In ſubjecting a number of fluids to the action of Galvaniſm, ſeveral facts have been diſcovered, which to me, at leaſt, are perfectly new, and which appear to throw ſome light on the nature and powers of this new influence.

"I ſhall, therefore, without any further apology, give a brief detail of ſome of the moſt important, hoping that they may prove acceptable to thoſe who are employed in the ſame purſuits.

"I ſhall not give any particular account of the apparatus employed, being a pile, and not differing materially from that in uſe. I ſhall only juſt obſerve, that it conſiſted of plates of zinc and ſilver, of about 1.6 inches ſquare, and that the number of each employed in the following experiments varied from 40 to 100, according to the power required.

"I found that a ſolution of the muriate of ammonia anſwered better for moiſtening the interpoſed papers than common water.

"When the machine was in full action, ſparks, which were perfectly viſible in the day time, could be taken at pleaſure, by making a communication in the uſual way between the extremities of the pile, and a ſmall report or ſnap could be heard; the ſhock given at that time was very ſtrong, and a gold leaf electrometer, placed in the circle of communication, was very ſenſibly affected: theſe circumſtances, ſome of which I believe have been already aſcertained by Meſſrs. Nicholſon and Carliſle, ſhew the ſtrong reſemblance of this influence to electricity. Theſe gentlemen have likewiſe diſcovered that Galvaniſm decompoſes water with much greater facility than electricity, but with phenomena ſomewhat different."

Mr. Cruickſkank then enters upon an account of his curious experiments; for which, on account of the length of this article, we muſt refer our readers to Mr. Nicholſon's Journal.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN my communication for February last, I announced an intention of coming forward in the spring with an 'Essay on the general Properties of the Fox-glove.' I pledged myself likewise to notice some Strictures of Dr. Drake on a letter I addressed to you in August last. The pressure of various avocations, however, have precluded me from fulfilling my engagements; but justice to myself, and to the cause I have espoused, require I should, without farther delay, perform the latter part of my intention.

When I first volunteered my services on this important subject, I did so, as I imagined, with the view of supporting the credit and real character of my favourite plant. I had for some years reared it with attention in my garden, and brought it to a degree of perfection which I had never observed it to attain in the natural state. My experience of its virtues in various diseases was extensive; and my success in hydrothorax by means of a combination of this herb, with certain other means, (which shall in due time be laid before the public) was great; I had reason to believe, beyond any former example. I had recently used it in some cases, (about twenty,) of consumption; therefore when its merits, in this last disease, began to be publicly discussed, I fancied myself in some degree prepared to bear a part in the discussion; at least, by adding my stock of facts and observations to the mass of evidence already before the public.

But, in candidly and faithfully detailing the results of the trials I had made, I was concerned to find I must necessarily differ in many important points from the respectable authors who had recently gone before me. They held out the most sanguine hopes of success, and seemed already to congratulate themselves, that an effectual remedy was at length discovered for that cruel scourge of mankind, consumption; while I endeavoured to prepare the public mind for more moderate expectations. Most earnestly do I wish their hopes had been realised, while mine proved otherwise. But, unfortunately for the human race, the reverse has been the event; subsequent experience having fully confirmed the truth of what I then advanced.

It is proper to observe, however, that Dr. Beddoes, one of the authors alluded to, with a liberality characteristic of great minds, finding he had exceeded due bounds in his encomiums

comiums on the Fox-glove, evidently from the high colouring of his correſpondents, has qualified his original language in a ſecond edition of his excellent work on Conſumption; and it augurs rather unfavourably to ſee his active mind again in ſearch of a remedy for this ſtill too generally fatal diſeaſe.

I obſerve, likewiſe, the ſentiments of the Monthly Reviewers, in their candid, judicious, and impartial analyſis of every thing that has recently been written on this plant, accord fully with mine. Not ſo, however, with Dr. Drake; whatever I have ſaid on the ſubject ſeems alike obnoxious to him: he not only objects to what I ventured to advance as facts, but repreſents me as diſcouraging the uſe of the Fox-glove from groundleſs fears of its deſtructive qualities. To him, therefore, permit me to addreſs a few lines, provided their inſertion in your truly valuable Journal be not likely in any degree to injure its juſtly-acquired reputation.

From the fortunate reſults of the two caſes publiſhed in the Contributions, &c. Dr. Drake thus concludes his encomium on the Fox-glove.

"I may, I think, without heſitation, affirm that the early exhibition of the *ſaturated* tincture in conſumption will in general prove ſucceſsful, and even when the diſeaſe is far advanced; provided the patient has but ſtrength enough left to endure a gradual depreſſion of the circulation, a reſult equally fortunate may be expected. That this can be done even in circumſtances of debility to an extent adequate to effect a cure, and without ſickneſs, languor, or loſs of appetite, the caſes appended will ſatisfactorily atteſt." (*Med. Contr. p.* 487.)

The principal grounds of my diſſent from this confident inference have already been ſtated. But this language, though ſtrong, fell far ſhort of that which was induſtriouſly promulgated by the Doctor's friends; and if my memory do not deceive me, by himſelf in a converſation we had twice on the ſubject. It had ſucceeded, it was urged, in every inſtance in which it was tried; and the moſt hopeleſs caſes were incapable of reſiſting its ſpecific powers. In vain did I aſſure them (his venerable predeceſſor Dr. Gibbon among the number) they deceived themſelves; that its powers were limitted even in the early ſtages of true conſumption. In anſwer to this it was ſaid, that I had been uſing the powder, from which, it was contended, "the wiſhed-for effect would ſeldom be produced." Every thing was to be effected by the wonder-working influence of the tincture: not even the tea of long life of St. Germain, the boaſted elixir of Caglioſtro, or the magnetic influence of Meſmer, were promiſed to atchieve more. But figure my aſtoniſhment on finding all the caſes in which it was ſaid to have

ſucceeded

succeeded dwindle away to two! I had myself been successful in more than double the number; and I thought it now incumbent upon me to come forward, as well to prepare the public mind for what my experience enabled me to say it was really capable of performing, as to state some particulars respecting its mode of preparation, exhibition, doses, and effects, which appeared to have been overlooked, or imperfectly understood, and consequently to have retarded the more general interdiction of this valuable herb. That I have done this has been productive of no regret to me. If I may be allowed to judge from the many private, and not a few public testimonies of approbation bestowed upon my remarks, although ushered into the world in the aukward and ungraceful dress too generally incident to hasty productions, I have reason to believe they have been favourably received by all except by Dr. Drake. For, says he, "some apprehensions are nevertheless entertained that injury may accrue to medicine, from the reports already published in favour of this plant. Your Journal for August contains an ingenious communication" (a communication which I would say ought to be burnt by the hands of the hangman, if the Doctor's objections were well founded,) "from Dr. Maclean, of Sudbury; in which, however, notwithstanding his own testimony to the good effects of the digitalis in consumption," he concludes a paragraph, and commences another, in the following words:" "*I see*, says he, "*the most serious evils begin already to result from its not answering the high expectations that have been raised.*"—"*Others begin to lose their confidence in it from similar failures; whereas, had it been brought forward with its true character stamped upon it, this would not be the case.*" (*Medical and Physical Journal*, *vol.* ii. *p.* 417). And afterwards, "I may also with confidence affirm that no evil of any magnitude can arise from the use of Digitalis in tubercular consumption, if properly exhibited, and that he who shall hasten to employ it early in the disease, will, in proportion to his promptitude, be a benefactor to mankind." And farther, "I am likewise well convinced that mischief may, and will, probably be produced by an injudicious and indiscriminate use of this active plant, &c." (p. 418).

I am really not solicitous to refute the imputations thus directly and obliquely conveyed, because I am persuaded, every one who has perused my remarks, and I am certain all those who are acquainted with my sentiments of the Fox-glove, will readily acquit me of them. Were it possible to attach such an interpretation to my words, the testimony of every practitioner within the circle of my regular rides, and many beyond it, with whom I have occasional intercourse, would positively con-

tradict

tradict it. For, whatever be the fate of this plant ultimately, the merit or demerit of bringing it into early and general use in this part of the country, I claim as attaching exclusively to me. I mean not, however, to infer that it had not been used by others before me; many had tried it in dropsy: but from want of success, owing to the causes already mentioned, I found it generally deserted; for when I fixed my residence in Sudbury, I recollect only two medical men who were in the habit of occasionally resorting to it; whereas, at this time, it is used by all, and this from a conviction of its extraordinary powers. And if the Doctor's scepticism in this respect be not invincible, he might easily persuade himself that my garden has liberally supplied the greater number with genuine leaves, not inferior in any degree to those of East Bergholt, where he loaded his botanical cart, and I may add, to those of any in the kingdom. Of this more hereafter. But, while the Doctor expresses his surprise that I do not assent to his conclusions respecting the almost specific virtues of the plant, in direct contradiction to my own experience, and, I venture to predict, to that of every man of extensive practice in this kingdom, he seems to have unluckily forgotten that he himself has, by a regular but insensible gradation, fallen into my opinion. "Of five cases," observes he, "of confirmed consumption, (including those of Grimes and Morris,) three have been perfectly cured. I have no expectation, however, that upon a larger scale, the proportion of fortunate to fatal result, would be what my experience has given." *(ib.* 268.) And again, in his last communication, "The experience I have now had, indeed, thoroughly convinces me that the digitalis will be found adequate to the cure of several cases of tubercular consumption, though advanced into the second stage; that it will for a time suspend the progress of the symptoms in almost every instance; and that in catarrh, in hæmoptoe, and in vomica, it will in some measure approach to what is commonly implied by the term specific, &c." *(ib.* 417). How different is this from the general tenor of the first paragraph transcribed! Had such language been originally used, the objections urged by me to this part of his Essay would never have existed, because it would have accorded with the sentiments delivered by me some months before, as may be seen in pages 115, 116, and 117 of the Medical and Physical Journal, vol. ii. to which I beg leave to refer the reader. He will perceive, that instead of saying any thing adverse to the use of the Fox-glove in consumption, I strenuously recommended it as a remedy which I trusted would be found by others as it had been by me, "the most efficacious that had hitherto been resorted to," although experience taught me, "its powers were limitted in the

the early ſtage." *(p.* 115.) Yet while I earneſtly encouraged farther trials, I was ſolicitous to guard the unexperienced in its uſe againſt the obvious conſequences of ſuch exaggerated encomiums. I ſaid, that I ſaw "the moſt ſerious evils begin already to reſult from its not anſwering the high expectations that had been raiſed." I ſay ſo ſtill; and in all human concerns it muſt neceſſarily be ſo. When a character has been ſtamped upon any thing which experience contradicts, diſappointment, often diſguſt, follows; and ſuch is the force of prejudice, that it ſometimes ſinks beneath its real value. This has unfortunately been the fate of the Digitalis with many, eſpecially thoſe whoſe minds were impreſſed with the belief, that "the exhibition of the ſaturated tincture would in general prove ſucceſsful in conſumption." But if the Doctor has at length (ſome months after his firſt Eſſay appeared) fallen into my views, why expreſs his ſurpriſe at my rejecting from my creed, what he himſelf no longer believes? It was not to what he intended to ſay, write, or publiſh, that my objections were urged, but to what he had actually ſaid, written, and publiſhed. Although born, and my early days paſſed in a country where the belief in the ſecond ſight was general, I had not yet learnt that happy gift of ſeeing through the impenetrable veil which conceals futurity.

That the Fox-glove has fallen into much greater diſrepute in ſome parts of this country, in conſumption, than it really merits, not only from the cauſes which excited alarm in me, but from the failures of the Doctor himſelf, is a fact too well known; and it is now no leſs difficult to convince ſome of thoſe who perſuaded themſelves that every caſe of conſumption would give way to this medicine, that it will ſucceed in any caſe, than it was before that it would not always ſucceed.

In vomica too, as well as in conſumption, "in which," ſays the Doctor, "it will, in ſome meaſure, approach to what is commonly implied by the term ſpecific; a word, however, which," he thinks, "ſhould be baniſhed from medicine;" if he had but patiently awaited the reſults of a more enlarged experience, he would have been more humble in his expectations. In this I have done much good with the Fox-glove, probably more than it has yet fallen to his lot to do, unleſs he has a great deal in ſtore; but it has ſometimes failed me in caſes where I had reaſon to be ſanguine. In the caſe of a maiden lady, 41 years of age, it approached indeed to what might be termed a ſpecific. Miſs Harriſon had a vomica ſo extenſive as frequently to expectorate a quart of thick offenſive pus, ſometimes tinged with blood, in twenty-four hours. It was, at one time, not only combined with ſerous effuſion in the cavities of the cheſt,

cheſt, but with general anaſarca, and her ſtrength ſo far exhauſted as to preclude all hope of recovery. But ſhe ſurvived this formidable attack, and ſeveral others for the laſt two years, by the aid of the Fox-glove. Her conſtitution has been ſo much ſhattered, however, by theſe repeated attacks, that, as might naturally be imagined, ſhe ſeldom enjoys what may be termed *good* health. She always has a phial of the tincture, or pills made of the powder, in her poſſeſſion, to which ſhe is frequently obliged to reſort, and invariably with relief. Other auxiliaries were not at the ſame time loſt ſight of.

"Now," continues Dr. Drake, "in the Contributions the Digitalis is related to have been given with the view to cure *tubercular conſumption*; and if by experience it has been found beneficial in hæmoptoe, and ſpaſmodic aſthma, does it follow, that ſhould it fail in the former complaint, it muſt neceſſarily fall into diſrepute in the latter diſeaſes? Surely Dr. M. ſhould have treated with contempt thoſe who are weak enough to think and argue in this manner." *(ib.* 417, *et ſeq.)* Dr. D. has unfortunately committed an overſight here. The caſes publiſhed by him were, he tells us, of *tubercular conſumption:* to theſe only did my remarks apply; hence, as he has been refuting objections created by himſelf, I hope he is ſatisfied with his own refutation; I have even poſitively affirmed that "Digitalis will be found highly beneficial in aſthmatic affections." *(ib. p.* 117, *vol.* ii.*)* Applying them, however, to conſumption, as they were done by me, I ſuſpect not only thoſe reſpectable practitioners he politely adviſes me "to treat with contempt for being weak enough, &c." but every one of experience who will candidly decide on the merits of the Fox-glove, would be diſpoſed to retort the charge of credulous weakneſs upon himſelf, for promiſing general ſucceſs from a remedy he had uſed only in two caſes! (in a diſeaſe too which had hitherto very generally reſiſted all the efforts of art;) if he did not fall under the imputation of a violation of the rules of good manners.

To his objections to the proportions of leaves to ſpirituous menſtrua given by me, and the ſtrength of the tinctures thus drawn, I have already ſpoken, *(ib. vol.* iii. *p.* 152*)*. And as he has been ſilent in his laſt reply, this point may, I preſume, be conſidered as decided. I ſhall beg only to obſerve, that the term *ſaturated*, on which he dwells with ſo much emphaſis, and to which, he ſays, tinctures made with leſs proportions than one ounce of the dried leaves to five of proof ſpirit, "can offer no claim," was never uſed by me, becauſe I conſidered it a vague and unmeaning term, as applied by Dr. Darwin and him, having aſcertained by repeated trials, that what was *ſaturated* in one temperature

temperature of their tincture was not so in another, from the great deposition which took place. But if by the term *saturated*, be understood that property by which the spirit retains *permanently* a given quantity of the active parts of the leaf, in every variety of temperature in which it is likely to be placed, (not that by which it does so for a given time, and in a certain temperature only) the tinctures made agreeably to my formula, have, I think, a much stronger claim to it.

By digestion for a week or more, a very important object is gained; half the proportions of the leaf being sufficient; and a larger quantity of the tincture, of equal, more uniform, and permanent strength, being obtained.

For a digestion of twenty-four hours, there is no precedent in the Pharmacopœia of the learned Colleges of London or Edinburgh; undoubtedly, because experience taught them it was not sufficient time for the spirit to extract and retain the active parts of vegetables. By their decision, in regard to the formula submitted for their consideration, will I readily abide. I now conduct the digestion in a cool temperature, by which I find the tincture is still less liable to variation.

Ample experience now warrants me in repeating what I formerly said, (*ib.* p. 52) that I have not been able to exceed ninety drops in the day, except in the instance mentioned. I find sixty drops in twenty-four hours in general fully adequate to place the strongest habits under its influence, of which many examples shall in due time be given.

A young woman, who was last summer cured of consumption, and is at this time in perfect health, was nearly poisoned by 66 drops in twenty-fours. She began with 120 drops in a half pint saline mixture, accurately measured, of which two table spoonsful were taken three times a day. The quantity of tincture was gradually increased to 176 drops: Before she began the mixture which contained this quantity, her recovery was rapid, and the habit was sensibly under the influence of the medicine. The first few doses occasioned sickness and vomiting, yet she persisted until the whole was consumed. A female relation now came to me, to say she was dying, having retched and vomited incessantly for three days, and that she was now so bad and so faint, they could not keep life in her." By means of solid opium made into pills with confectio aromatica, (for every thing liquid was instantly rejected) warm anodyne embrocations, and an anodyne glyster, she was in a few days pronounced in safety, and a fortnight more completed the cure. The patient being at a distance from Mr. Rainbird, of Colne in Essex, (who attended with me,) and myself, she was rigidly enjoined to desist, as soon as the usual effects of the

medicine appeared, until ſhe heard from either of us; but, as too frequently happens, our injunctions were diſregarded. The tincture was of my own preparing.

In a variety of caſes of the moſt acute and painful ſuffering, which ſhall, I truſt, in due time be ſubmitted to public inſpection, I have ventured to preſcribe 30, 35, and at one time, 40 drops at a doſe; the two former repeated twice a day, and very generally with the happieſt effects. In ſome inſtances, the habit has, in a few minutes, felt its ſalutary influence; and in general, the ſtomach, pulſe, and head, have been affected in twenty-four and forty-eight hours. In urgent caſes, as relief muſt be ſpeedy, ſo the doſe muſt be large, and proportioned to the urgency of ſymptoms.

In formidable caſes of meaſles, when the urgency of the cough, the quick, difficult, anxious, or laborious reſpiration, with a high fever, denote a dangerous diſeaſe; eſpecially if from the hiſtory of the family there be reaſon to apprehend a ſcrophulous or phthiſical diatheſis, but in which blood-letting, or the exhibition of opium, may be thought equivocal or hazardous, I beg, in a particular manner, to direct the attention of medical men to full doſes of the Digitalis. In ſuch caſes, fever is allayed, expectoration and all the other ſecretions promoted, reſpiration relieved, and the bowels relaxed, by its means, whereas the very reverſe is frequently the conſequence of opium. Hence its ſuperiority in many inſtances.

In the firſt caſe of meaſles in which I preſcribed it, I had every reaſon to attribute the ſafety of the patient entirely to its uſe. The ſame perſon, Mr. E. Oxley, of this town, was relieved in a few minutes, and permanently cured, by full doſes of it, of a moſt violent hemicrania, attended with intenſe heat and thirſt, with a feeble pulſe which beat about 140 ſtrokes in a minute. Laudanum in large quantities, and other means, were adminiſtered with evident aggravation of every ſymptom before I ſaw him. Being in the country when ſent for, they "thought," to uſe their own words, "he would go from his ſenſes before I arrived."

In a moſt violent caſe of external and internal hæmorrhoidal affection, now under my care, 40 drops gave almoſt immediate relief, and the patient is recovering rapidly by 35, evening and morning. When he firſt came to me, his countenance was pale and ſallow, his ſtrength and fleſh much exhauſted; he walked with extreme pain and difficulty; his pulſe was quick and ſmall, and his appetite impaired. In a week the contraſt was very ſtriking.

The diſeaſe had reſiſted judicious treatment for five weeks.

But in many other inſtances, as well as in conſumption, the

practitioner

practitioner must be prepared for disappointments, by this, as by every other human means. Certain diseases are beyond the reach of our art; and this may probably be wisely intended by the Author of our being, in order to impress our minds with our own insignificance. If the tinctures be properly prepared, of the genuine leaf, they will not be pale as brandy, but of a darker tinge than laudanum, of which two vials now before me exhibit a striking proof.

I have this year dried, and am now drying, several pounds of the leaves of my own garden, on a net properly suspended in a chamber through which a current of air passes, but from which the rays of the sun are excluded; and, especially when large quantities are prepared, this method, as being simple, easy, and certain, is perhaps preferable to any yet adopted.

Most cheerfully do I acknowledge myself indebted to Dr. Drake, and the celebrated author of the Zoonomia, for directing the attention of medical men more immediately to the tincture of Fox-glove. By means of this preparation I have been enabled to alleviate human misery to a much greater extent than would otherwise have fallen to my lot to do. I can now readily furnish a number of distressed objects, who come daily to implore my assistance, with a small vial; whereas I had no means of providing them with the infusion, or pills made of the powder. In this way, it is one of the highest gratifications of my life to reflect, that I have done much good, and, I trust, may be enabled to do much more. When a decoction, or strong infusion is deemed necessary as an external application, my garden immediately supplies them.

An interesting case in point, now comes to my recollection: —— Bridget, an infant a year old, was brought for my advice the 21st of November last. A highly inflamed scaly eruption, discharging a thin ichor, with here and there painful pimples, all over the head, had harassed it from its birth. Medicine had been resorted to in vain. I gathered a handful or two of fresh leaves, with which I directed a strong decoction to be made, and the head to be bathed with a little, evening and morning. It was brought to me again the 27th, nearly well; and on the 2d of December free of complaint. It gave me pleasure to observe, soon after this, in Dr. Ferriar's excellent treatise, similar happy results. In a case of true *tinea capitis* it failed with me; but in itch it seems an effectual remedy, of which the first hint was given me by Mr. Anderson, an intelligent practitioner of this place.

The principles on which I give the preference to the tincture, in certain cases, have been explained, (*ib. vol. iii. p.* 152, *et seq.*) and are very different from those on which Dr.

 Drake

Drake prefers it. He has thought proper to condemn the powder as a preparation, "by which the wished-for effect in consumption would seldom be produced," without trying it in a single instance! This extraordinary assertion, without the shadow of proof, I have already called to his recollection, (*ibid.*) and he has not attempted to contradict it; nor can he contradict it, without impeaching his own veracity; for he expressly tells us, the tincture was the only preparation used in all the cases he has brought before the public. By what authority then, or on what grounds, does he presume to pronounce his anathema against a substance he has never put to the test of a trial in consumption? A substance too which has been used with success by others in the disease; and which has, under the eye of the highest medical authorities in this kingdom, produced every effect on the human body of which the tincture is capable.—A word or two may, probably, tend to unravel this mystery.

In one of the cases alluded to, in his communication for Oct. last, (*ib. vol. ii. p.* 267) he tells us, "that the first, a young man about twenty-eight years of age, had been under the care of two physicians in the neighbourhood previous to his being called in." After drawing the history of the case, I doubt not, very correctly, the reader is informed in a note these physicians were "Dr. M'Lean and Dr. Clubbe, by whom the Digitalis had been administered in the form of powder; the wished-for effect, however", continues he, "can seldom be produced by the powder, &c." "The tincture was pushed to 120 drops in twenty-four hours." But after being three weeks under "its full influence, with the symptoms nearly stationary, he became averse towards prosecuting it farther; nor could I persuade either him or his friends to permit a more extensive trial. He died in five or six weeks after." What inferences are intended to be deduced from these premises? They appear to me obviously these:

1. That the powder having been administered by two physicians, had a fair trial; but,

2. That as the "wished-for effect is seldom produced by the powder," it failed as a natural consequence.

3. That had the tincture been given by these physicians, success would most probably have attended it, as general success was at this time promised from it.

4. That had the tincture been persevered in during the Doctor's pleasure, a happy result would most likely have been the consequence; for the failure is not attributed to want of efficacy of the medicine, but want of resolution of the patient, "the symptoms being nearly stationary." And,

Lastly,

Laſtly, That theſe two phyſicians fell under the heavy imputation of neglecting the only means of ſaving their patient.

I anticipate very different concluſions, however, from the candid reader, on peruſing the following brief ſtatement.

My firſt viſit to Mr. Boutelle, of Polſtead, the ſubject of this caſe, I find by my notes to have been on the 24th of March, 1799. In addition to the ſymptoms detailed by Dr. D. he was copiouſly expectorating blood, generally florid, which he had done more or leſs from the commencement of his diſorder. I ſaw him again on the 30th, and I perceive by my memorandum, I thought him ſomewhat better. The Digitalis was now preſcribed, as follows:

℞. Fol. dig. p. pulv. gr. viij G. arab. p. gr. vj. Confect. aromat. q. s. ft. Boli no. octo. ſumt. 1 ſing. noct. et matutinis.

Saline mucilaginous draughts were likewiſe preſcribed every ſix hours.

The next tidings I had of him, from Mr. Salter the attending ſurgeon, were by letter a week after. I was informed that eight pills only were taken; that the laſt two diſordered his ſtomach and head much; that he poſitively refuſed perſevering in them any longer, as he thought himſelf worſe the whole time of taking them; that his friends and himſelf became reſtleſs and impatient, and wiſhed to have further advice. I was much ſurpriſed to find, from Mr. Salter's letter, that the hæmoptoe was evidently aggravated during the uſe of the medicine. Having occaſion to meet Dr. Clubbe of Ipſwich, in conſultation, the following day, the 9th of April, in the neighbourhood, we were deſired to ſee Mr. B. and it is but juſtice to Dr. Clubbe's ſagacity and diſcernment, to declare, that he pronounced the caſe at once hopeleſs. I own there appeared to me ſtill ſome ray of hope. The Digitalis was not again preſcribed; for, as hinted on a former occaſion, Dr. C. had been extremely unſucceſsful with it. I ſaw the patient no more.

As we promiſed nothing, ſo we neither deſired, nor were ſolicited, to repeat our viſits. At this time the miraculous cures that had been, and were to be, performed by the tincture, were loudly thundered forth from the trumpet of fame. Hadleigh was within a few miles; the pleaſing ſound ſoon reached the ears of the patient and friends. They naturally ſought relief where they were taught to believe they were certain of finding it; "for the tincture had not as yet failed in a ſingle inſtance." The Doctor was called in. What were his promiſes may be eaſily conjectured. Of what followed we are appriſed. On the above ſtatement I ſhall only offer one or two further remarks.

When a favorite remedy fails, it unfortunately happens that the

the author generally finds ſomething or other at hand to account for the failure. Every thing unfavorable to his purpoſe is ſcrupulouſly kept in the back ground, while every favorable circumſtance is exhibited to view, as evidently happened in the preſent inſtance. "The powder was adminiſtered by two phyſicians," that is, it had a full and a fair trial; but it was not a preparation ſuited to the caſe; hence, "the wiſhed-for effect could not be produced." But had the tincture been at firſt uſed, from which, on the Doctor's authority, general ſucceſs was promiſed, or had it been perſevered in afterwards, for "the ſymptoms were nearly ſtationary," the reader is left to infer that the caſe would have terminated otherwiſe than it did. The conſtitution of this patient, as I have ſince more than once experienced, was in a particular manner ſuſceptible of the noxious effects of the Fox-glove. Small doſes diſordered every organ and function, and even encreaſed the flow of blood from the veſſels of the lungs. I firmly believe then, that it was one of thoſe unfortunate caſes which would have reſiſted the ſalutary powers of the tincture of Fox-glove, and every other human aid, although judiciouſly adminiſtered from the firſt week of his illneſs.

If the Doctor has condemned what he has not uſed, it is not ſurpriſing that he ſhould have diſapproved of what he has not ſeen. His objections to, and mode of arguing againſt, what I have ſaid on the culture of the Fox-glove in my garden, are of a moſt peculiar caſt. I affirmed, (*ib.* p. 121.) that in conſequence of the diſappointments experienced from the bad quality of the leaf the firſt few years I preſcribed it, "I had for ſome years cultivated the plant in my garden, whereby I had reaſon to believe its virtues were improved; that I was certain they were not impaired." To which the Doctor thus replies, "With regard to the plant itſelf, I can aſſure Dr. M. he is deceived in ſuppoſing it to acquire greater vigour from cultivation in a garden. Nineteen times out of twenty he will find the reverſe to be the event. The Digitalis delights in an elevated, a light and gravelly ſoil; on a *fat* and *denſe* mould, in *low* and *damp* ſituations, and where *ſtrata* of *chalk* abound, it always degenerates, becomes dwarfiſh, and of a paler green. Dr. M's houſe at Sudbury, is certainly ſituated low, not far from the river, in the immediate neighbourhood of chalk-beds; on ſuch a ſoil, it is evident none of the habitudes of the plant can be gratified; and by the uſual modes of improving a garden, the ground will be rendered ſtill leſs fit for promoting its growth and ſtrength, &c." (*Ib. vol.* ii. p. 421.)

I own I was not aware of the ſhadow of an objection to what I ventured to ſay on this head. I fancied to myſelf the facts above

above ſtated, had been aſcertained both by my medical friends and me, after repeated trials, beyond the poſſibility of doubt. But Dr. Drake ſeems to poſſeſs the happy talent of acquiring that knowledge by intuition, which others cannot attain by experiment and attentive obſervation. For, as far as I know, he has never been within the walls of my garden; he has never ſeen the Digitalis growing in it; nor has he put the virtues of the leaf, thus reared, to the teſt of a ſingle experiment!

The reader will perceive that I ſpoke only of my *own* garden; and had he *poſitively* contradicted my aſſertion, I could have anſwered and confuted him in *one* word. Aware however, I preſume, of the conſequences of one gentleman calling in queſtion the veracity of another, without ſubſtantial evidence, he prudently avoids this, and, with a ſpecies of ſophiſtry that may impoſe on weak and ſuperficial minds, but too ſhallow, I truſt, not to be at once detected by men of diſcernment, ſhifts the ground, and fabricates a ſoil and ſituation for my garden, which exiſt only in his own imagination; and with an air of profound ſagacity tells us, the Digitalis will not thrive in ſuch a ſoil or ſituation. The terms "on a fat and denſe mould, in low and damp ſituations," &c. are not to be found in my remarks; and ſuch a ſoil and ſituation are certainly not thoſe of my garden. The gardeners and farmers of this country would laugh at his definition of the ſoil of our gardens and fields, and bluntly tell him he was totally ignorant of what he claims a familiar acquaintance with. They would aſſure him, they are neither chalky nor calcareous, nor denſe nor damp; that wherever chalk-beds are found, the chalk is ſeveral feet, in general many yards, beneath the ſurface, a thick ſolid ſtratum of gravel intervening; that neither the plough nor the ſpade ever reach it, except where the natural ſoil has been removed to fill up inequalities, as I have obſerved in a field in the pariſh of Henny, in Eſſex, a few miles from Sudbury; that although the ſoil may vie in fertility with any in the kingdom, it is rather of a looſe than of a denſe and ſolid texture. This is the ſum of the information I have received from the gardeners and farmers, and accords with my own obſervations; and few have traverſed its ſurface, for ten years, more frequently. But, for argument's ſake, admitting the ſoil, and every other circumſtance, to be as the Doctor repreſents them, does he imagine that any one, finding the Fox-glove growing luxuriantly in an elevated ſituation, in a light gravelly ſoil, would be ſo conſummate a blockhead as to plant it in "a fat, denſe mould, in a low, damp ſituation, &c?" I can aſſure him we have no ſuch in this part of Suffolk. Our gardeners here have more intelligence than he ſeems aware of; they would inform him,

ſtrenuous

ſtrenuous as he is for leaving this plant in the hands of nature, that both this, and hundreds of others, originally growing in the fields, are now reared in gardens, in much higher luxuriance than in their original ſtate; that although ſome vegetables degenerate and die, yet many others are improved in all their properties by ſuch removal; that one of the principal ends of their art is, to improve upon Nature when ſhe is deficient, to imitate her when ſhe has attained the higheſt degree of perfection of which ſhe is capable.

Whoever has attended to the Digitalis in its natural ſtate, muſt have ſeen, that in the ſame field or hedge-row, ſome plants are in full health and vigour, while others are weak and dwarfiſh. Nature often diſtributes her gifts, for wiſe and ſalutary purpoſes, with a liberal, often with a ſparing hand; and the intelligent botaniſt, even the moſt ignorant gardener, well knows that the ſoil and ſituation in which ſhe has placed her numerous offspring, animals as well as vegetables, are not always the moſt congenial to them. But, far be it from my wiſh to adviſe the officious interference of art, where ſhe is bountiful of her gifts. It was not until I had experienced repeated diſappointments, that I was induced to cultivate this valuable plant within my immediate reach, and ſtrenuouſly to recommend it to many of my medical friends; and from the additional confidence which farther experience inſpires, I not only venture to repeat, what I have already ſaid in reply to the Doctor's objections, (*ib. vol. iii. p.* 151, *et seq.*) but affirm, that I never ſaw it in any place, and I believe it never did, attain to ſuch beauty and perfection, as well as height. I have heard of its growing ſix and ſeven feet high in its natural ſtate. I never ſaw this: The imagination of thoſe who argue like Dr. D. may readily magnify it to this height, while it would reduce that of my garden to dwarfs,—to mere pigmies. Dr. Withering ſays, " the ſtems are about four feet high." I have, I think, ſeen it higher than this: the talleſt of my produce, by accurate meaſurement lately, were five feet ten inches high; but thoſe of three and four feet throw out the ſtrongeſt and coarſeſt leaves. They have one peculiarity likewiſe, which I have not obſerved in the wild plant, which adds much to their beauty and elegance, and which they ſeem to derive from artificial culture. They have from four to ſix ſmaller ſtems, ſome of which are two feet in length, with ſeveral bloſſoms iſſuing from the principal ſtem, at twelve and eighteen inches from the ground. Theſe come later, and the bloſſoms continue longer than the principal ones: they are not in Withering's plate, or in any I have ſeen. Are they uſual?

The ſtems of the white plants are more delicate and ſlender; the

the leaves of a paler green, of a ſmoother texture, and in general not ſo large as thoſe of the purple. I have diſcovered no difference, however, in their virtues. The two plants form a moſt beautiful contraſt; and as there is a ſucceſſion of bloſſoms for ſome weeks, few contribute more to the ornament of the flower garden.

'Were it neceſſary to adduce farther facts, in oppoſition to general aſſertion, I might add, that two pariſhes (Higham and Lawford) adjoining to Eaſt Bergholt, in which the Doctor loaded his cart with Fox-glove, ſupplied me moſt liberally laſt ſummer, on which many trials were made.

What I have ſaid of the neceſſity of free expoſure to the ſun and air, &c. is not only ſtrictly correct, but a general law in regard to vegetables, with as few exceptions, at leaſt, as any other. When gathering the plants at Lawford Hall, on the grounds of my much reſpected friend, E. Green, Eſq. of that place, hundreds were rejected, becauſe they were ſo ſhaded by trees, and ſo ſurrounded by weeds and other plants, as, from want of ſun, air, and nutriment from the earth, not to arrive at a quarter of their natural ſize. They were thin, pale, and ſickly; ſome of the ſtems were tall, but very weak and ſlender. In a long and painful attendance lately on my truly worthy friend, the late Rev. Dr. Preſton, of this country, a ſtriking inſtance of this occurred to me: In his beautiful ſhrubbery I had often admired the luxuriant growth of the Fox-glove; of late, from cauſes unneceſſary to ſtate here, the grounds had been neglected, and were conſequently over-run with weeds; in vain did I for ſome time look for my old acquaintance; at length, however, I found a few weakly plants concealed by the weeds, without any *ſtems or bloſſoms*.

[To be continued in our next.]

Celeberrimo Doctori G. PEARSON, &c.
S. P. D.

Med. et Chir. Dr. J. C. W. JUNCKER,
Profeſſor Medicinae in Academia Regis Boruſſorum Halenſi Ordinarius, &c. Halae Magdeburgicae, d. 17 Junii, 1800.

COGNITUM forte habebis, vir celeberrime, ſocietatem medicorum Germaniæ ac Helvetiae mecum conveniſſe, ut miſeriis humanis, quae a veneno varioloſo proveniunt, limites,

quoſcunque ponere liceat, ponantur. En! brevem hujus inſtituti hiſtoriam.

Anno 1791, in urbe noſtra morbi graſſabantur maligni, ſic dicti nervoſi. Inſitio variolarum hoc tempore temere inſtituta (neglectis nempe regulis, quae ad evitandam mali propagationem ſpectant) anſam praebebat epidemiae variolarum. Haec invaſit homines 2151; ex his necavit 430; alios 7 videndi facultate privavit, et multifaria denique mala reliquit in hominibus 280. Dicto anno et partim etiam anno ſequente ſingula urbis noſtrae aedificia quin et pagos proxime vicinos exploravi reſpectu variolarum; auditorum nempe 20 auxilio adjutus. Quot et quantas miſerias malum varioloſum ejuſve inſitio temere inſtituta induxerit, hoc modo (dicta nempe exploratione aedificiorum, &c.) ſatis exacte poterat cognoſci. Mala, quae tum vidi, animum moverunt mirifice. Jam fretus multorum (potiſſimum medicorum) promiſſis, &c. fore ſperabam, ut noſtrae urbis incolae optimis conſiliis, quibus malum varioloſum vel mitigari vel arceri poſſet, aures praebituri ſint benevolas. Egregio exemplo noſtrates præire alioſque vicinos ad imitationem utiliſſimam, tali exemplo praegreſſo, eo facilius movere poſſe putabam. Edidi in hunc finem primis menſibus anni 1792, librum de variolis, in uſum praeprimis Halenſium conſcriptum.* At ſpes me fefellit. Paulo poſt, cum hiſtoriam morbi cujuſdam (Doctoris nempe Bahrdtii) deſcriberem, † querendum jam erat de auxilio medicorum quorumdam circa variolas denegato, &c. (Cfr. libell. infra laudatus, No. 2, p. 54.)

Seriori inſtituta hac de re meditatione, aliam viam incedendam et ad omnes reliquos, quamvis remotiſſimos, *humanos* tamen medicos, nec non ad magnates quoſcunque, &c. ſenſim confugiendum eſſe, ſentii. In libello itaque anno 1795 edito,‡ medicorum Germaniae attentionem dirigere ſtudui ad majus quoddam

* *J. C. W. Juncker's*, &c. Gemeinnützige Vorſchläge und Nachrichten über das beſte Verhalten der Menſchen in Rückſicht der Pockenkrankheit. *Erſter Verſuch.* Halle 1792. Gr. 8. ſ. 236 und 96. (Jetzt in Commiſſion Halle bey Hemmerde. Pr. 1 thaler.)

† Etwas über die Weinbergſkrankheit des verſtorbenen Doctor Bahrdts und ähnlicher noch lebender Kranken. Den Nichtärzten zur freundſchaftlichen Warnung mitgetheilt von Dr. *J. C. W. Juncker*, &c. Halle in Commiſſion bey Hemmerde, 1792. Kl. 8. ſ. 55. Pr. 5 groſchen.

‡ Dr. *J. C. W. Juncker's*, &c. Gemeinnützige Vorſchläge und Nachrichten über die Pockenkrankheit. Für Deutrchlands Aerzte. Ein Vorſchläge aus der Volckſarzneywiſſenſchaft. Halle bey Hemmerde, 1795. Gr. 8. ſ. 67. Pr. 5 groſchen. (Item ſub titulo: Gemeinnützige Vorſchläge. *Zweyter Verſuch.*)

quoddam de variolis opus proxime proditurum, in quo omnia, quae huc pertinent, uberius essent illustranda.

Id quod prodiit sub titulo inferius laudato,* anno 1796. In hoc opere viam designasse mihi videor, qua quidem medicis illam incedentibus certissime datum foret, successivam praeparare et ad finem perducere variolarum exstirpationem; et 84 objectiones, huic exstirpationi oppositas, refutavi, ad viam nempe dictam respiciens. Simul autem ex omnibus Germaniae medicis petii, ut, quidquid circa hanc viam monendum esse ipsis videretur, mecum communicarent.

His precibus jam nunc multo plures satisfecerunt medici, quam 300; inter quos plures eminent nostri instituti egregie participes.—Plures simul nummos dono dederunt, unde enatum est, quod optaveram, *aerarium contra variolas generale* seu *commune* (h. e. variolarum remotioni in universum dicatum. Neque enim *singulis* quibusdam, sed *omnibus* locis illud aerarium dicavimus, illudque opponimus aerariis *localibus*, quibus fore speramus, ut in hoc illove *loco* incolae ejusdem aliquando sibi prospiciant. Dictum vero *aerarium commune* jam nunc circiter continet duo millia imperialium, numeratis nempe simul nummis subscriptionis lege promissis.)

Ut vero judicia medicorum facilius transmitti potuerint, ex omnibus fere, qui Germaniam regunt, magnatibus precatus sum, ut, finito quovis anno, numerum eorum, qui praeterito anno in suis regionibus mortui essent variolis, aliaque, quae variolarum respectu memoratu digna viderentur medicis seu verbi divini ministris, colligenda et haec denique omnia collecta sive summatim excerpta mihi transmittenda curarent. Responderunt his votis plurimi iique potentissimi Germaniae principes; Imperator v. g. Germaniae; Rex Borussorum; Elector Saxoniae; Duces Saxoniae; Magnates Harsiae; Megapolitani; Auhaltini, aliique multi.

Tot itaque adminiculis instructus inde ab anno 1796 *periodicum opus* edidi, ut quovis anno vel semestri innotesceret, quales in hac causa progressus fecerit societas medicorum. Cujus quidem operis huc usque prodierunt volumina septem.† Volumen

* Dr. *J. C. W. Juncker*'s, &c. Gemeinnützige Vorschläge wider die Pockenkrankheit. *Dritter Versuch.* Für möglichst alle Aertze, die der deutschen sprache kundig sind; zur sammlung ihrer Gutachten hierüber. Halle bey Hemmerde. Gr. 8. s. 502. Pr. 1 thaler 8 groschen.

† Archiv der Aerzte und Seelsorger wider die Pockennoth. *Erstes Stück.* Michaelmesse 1796. Heraus gegeben von Dr. *J. C. W. Juncker*, &c. Leipzig in der Weygandschen Buchhandlung, 1796. Gr. 8. s. 296. Pr. 18 groschen. *Zweytes Stück.* Ostermesse 1797. Gr. 8. s. 306. Pr. 22 groschen. *Drittes Stück.* Michaelmesse 1797. Gr. 8. s. 218. Pr. 18 groschen. *Viertes*

Volumen octavum prodibit tempore nundinarum Lipsiensium, quae sequuntur. Novo, quod proximum est, seculo idem opus periodicum, titulo paululum mutato, continuabitur. (*Neues* Archiv wider das Pockenelend, &c.)

Jam vero comparatis Germaniae medicorum suffragiis, meum esse putavi, in unum colligere, quae omnibus istis medicorum consiliis consentanea sint, quaeque *nostris* temporibus et adeo diversis locis Germaniae, &c. respondeant. *Et haec quidem omnia* (missis ceteris, in quibus medici dissentiunt, &c.) *cum omnibus Germaniae magnatibus praesenti anno* seu certe primis futuri anni mensibus communicabo.

Responsiones magnatum exspectamus sequente anno 1801. Quibus collectis novum de successiva variolarum exstirpatione opusculum lingua Latina aeque ac Francogallica conscriptum, *omnibus* itidem *exteris*, quibuscunque licebit, *magnatibus* transmittendum curabit societas medicorum. Brevem hujus libelli mentionem jam feci in *Mémoire adressé au Congrés de Rastadt, concernant la petite vérole* mense Maii 1798. De his autem et similibus suo tempore pluribus te reddam certiorem.

Utinam et tibi placeret, vir praestantissime, *finito quovis anno* mecum communicare, quae tibi nuperius digna visa sint memoratu circa variolas *in patria tua!* (quinam v. g. libri huc pertinentes anno quocunque praeterito prodierint? quae *remedia* horrendo huic veneficio opposita sint *vi legum* vel *suasu medicorum? &c.*

Et hoc quidem *primum* est, quod ex te precor. *Delectes me hujus generis epistolis finito quovis anno!*

Alterum vero momentum, cujus causa has litteras conscripsi, *insitionem* spectat *variolarum vaccinarum.* Legi de his sequentes libros: 1. An Inquiry into the Causes and Effects of the Variolæ Vaccinæ, &c. by *E. Jenner*, Lond. 1798. (vers. in linguam Germanicam, *Ballhorn*, Hannover, 1799; in linguam Latinam *Careno*, Vindob. 1799;) 2. Further Observations, &c. by *E. Jenner*, Lond. 1799. 3. An Enquiry concerning the History of the Cow-pox, &c. by *G. Pearson.* London, 1798; (vers. in linguam Germanicam, *Küttlinger.* Nürnberg, 1800;) 4. Reports of a Series of Inoculations for the Variolæ Vaccinæ, or Cow-pox, &c. by *W. Woodville.* Lond. 1799. (Vers. in linguam Germanicam, *Friese.* Breslau, 1800.)

Jam

Viertes Stück. Neujahrsmesse 1798. Gr. 8. s. 291. Pr. 1 thaler 4 groschen. *Fünftes Stück.* Ostermesse 1798. Gr. 8. s. 320. Pr. 22 groschen. *Sechstes Stück.* Michaelmesse 1798. Gr. 8. s. 304. Pr. 1 thaler. *Siebentes Stück,* Michaelmesse 1799. Gr. 8. s. 352. Pr. 1 thaler 4 groschen.

Jam vero in votis eſt, ut infantibus quibuſdam Halenſibus, quos in deliciis habeo, aeſtate hujus anni ſive ejuſdem autumno inſerere queam variolas vaccinas. Ne vero decipiar et materiem forte accipiam a variolis vaccinis *ſpuriis* depromptam aliove reſpectu ſive perniciosam ſive non ſatis efficacem: *Ardentiſſime ex te precor*, Ut mihi, quam proxime fieri id queat, tranſmittendam cures materiem his votis apprime conſentaneam. Quem igitur in finem ſollicitus, quaeſo, ſis de homine ceterum *ſaniſſimo, qui poſt inſitionem materiei*, e vacca puſtulis vaccinis laborante *recenter* depromptae, variolas vaccinas *veras leniter* (ſueta, volo, non anomola ratione) perpeſſus ſit. Materiem ex HOC HOMINE depromptam mihi, precor, tranſmittendam cures, vaſe *v. g.* vitreo bene obturato et capſula inſuper *v. g.* lignea munito incluſam. In ſchedula annexa tua manu benevole teſteris, materiem hoc vaſe incluſam *vere talem eſſe qualem optaverim*, eamque igitur ita eſſe comparatam, ut ab inſitione ejuſdem (ceterum rite inſtituta) ſoliti et ſalutares eventus expectari queant.

Quibuscunque impenſis hujus generis labor anſam forte praebiturus ſit, gratiſſimo animo compenſabo.—Si forſan *cautelae quaedam*, quae *inſitionem hanc* ſpectant, ad notitiam tuam *nuperius* veniſſent, quarum in libris ſupra laudatis mentio facta *non* eſt: *has quoque cautelas* mecum benevole communices!

Vale. Fauſta quaevis Tibi apprecor!

P. S.

Adreſſe.

Litteris Tuis extus quaeſo inſcribas:

Dem Herrn,

Dr. *J. C. W. Juncker*,

Profeſſor der Medicin auf der Koeniglichen Preuſſiſchen Univerſität.

Nebſt einem Päckchen, bezeichnet:
Medicin aus London, an H. P. J. zu Halle.*

Zu HALLE, (bey *Leipzig.*)

* Haec verba inferius et ſiniſtrorſum poſita ſignificant; litteras tuas comitari vaſculum, quod contineat medicinam Londinenſem, et quod ſignatum ſit verbis; an H. P. J. zu Halle.

The foregoing Letter was ſent to Dr. PEARSON, and by him given to the Vaccine Pock Inſtitution, from which we have permiſſion to inſert it.—EDIT.

To

To Dr. BATTY.

Dear Sir,

IF you will insert the following definition of the Cow-pox, in the next Number of your useful Journal, you will add another obligation to the many attentions already shewn to

Your obliged humble servant,
RICHARD DUNNING.

Plymouth Dock,
July 7, 1800.

VACCINA JENNERIANA.

Morbus Vicarius, potiusve Processus succedaneus, mirifico Variolam cèrte præveniendi, immò (quod veresimilius sit) penitùs abolendi fungens munere, neve ullam futuræ Saluti vel minìme adversam Sequelam illaturus.—Cuticulari Vulnusculo Vaccinitate appositâ, Tumor parvus, *localis*, *vesiculosus*, *carbunculosus*, inde exortus, Febrem ephemeram plerùmque quàm levissimam et omnìno incontagiosam incitans; paucis Papulis, subìnde, sed *perràro*, *nec necessàrie* supervenientibus, tertioque ferè die prorsus evanescentibus.

To Dr. BRADLEY.

Dear Sir,

I Thank you for the Cow-pox matter, with which I began to inoculate on the 6th instant; and have the pleasure to inform you, that most of the children have taken the infection. I enclose a case, which, with the remarks, I hope you will think worthy of a place in your valuable Journal. I am,

Dear Sir,
Your obliged and faithful servant,
JOHN MILNER BARRY, M.D.*

Cork, June 18, 1800.

HYPOTHETICAL theory too often opposes the progress of knowledge, by withdrawing the attention from facts, and discouraging a disposition to observe the phænomena of disease, and the action of medicines, without which it is impossible to make any advances in medical science. But, strange as it may appear,

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* In No. XVI. p. 504, this gentleman's name was, by mistake, printed Bany.

appear, it is not among men, who devote a great portion of their time to reading and ſpeculation, that we find thoſe, who are moſt liable to grant this unwarrantable preponderance, to conjecture over facts. Men who profeſs to be governed entirely by experience, who affect to deſpiſe theory of every kind, however legitimate, and who indulge an unbounded ſcepticiſm about every new improvement, are the genuine ſlaves of hypotheſes, which have been long diſcarded from the creed of every enlightened practitioner. When ſuch men attempt to theorize, that is, to draw general inferences from what paſſes before their eyes, which they are forced to do as well as their better-informed brethren, their reaſonings are compoſed of a confuſion of hypothetical opinions and expreſſions, imbibed without examination in their early ſtudies, which, in the lapſe of years, while ſcience advances, preſerve a ſtrong and undivided power over their minds. They are the firſt to cenſure, and the laſt to adopt the ſucceſſive improvements in their profeſſion, and are the more dangerous to the cauſe of ſcience and humanity, becauſe they uſually poſſeſs a ſtock of arguments, which favour the natural indolence, and neceſſarily limited information, of the generality of mankind.

The opinions relative to the effect of heat and cold on the human body, which long prevailed in the ſchools, were intirely drawn from the analogy of unorganized matter, and the *vital principle* was overlooked, or wholly diſregarded. It gives me pain to obſerve, that the doctrine of relaxation, which is a neceſſary conſequence of this falſe analogy, is ſtill admitted by many practitioners, and has prevented the more general uſe of the warm bath, which, from the united teſtimony of phyſicians of the greateſt reſpectability, is not only a ſafe, but a moſt efficacious remedy. The following caſe, beſides poſſeſſing ſome claims to notice in other reſpects, muſt furniſh the advocates for this exploded hypotheſis, with a ſatiſfactory refutation, and may facilitate the application of it, in diſeaſes of extreme debility, where other means have been tried without ſucceſs. I hope this will be a ſufficient conſideration to excuſe its publication, as well as the previous remarks.

Miſs Ellen M'Sweeny,* æt. 21, July, 1799, after much faſting

* This lady belongs to a religious ſect, which in this part of Ireland, expects from its votaries, a rigid compliance with all the rules of the church. The ladies, as may naturally be ſuppoſed, are much more ſtrict in the obſervance of faſt days than the men, who differ very little in this reſpect from the Continental Catholics. It will ſcarcely be believed, but is notwithſtanding

fasting and abstinence during last Lent, sat up for some nights with a friend, who was ill of a fever, which terminated fatally. Grief, fatigue, want of rest, together with the previous debilitating effects of fasting, or a diet not sufficiently nutritive, brought on slight hysterics, which were followed by frequent vomiting after food or drink. I first saw her in the month of April last, which was about five or six weeks after the first attack. She complained of a dull pain in the region of the stomach, extending to the back; she threw up every thing she swallowed during the day, but sometimes retained about a pint of whey at night, if she dropped asleep immediately after taking it. Her strength and appearance were not yet materially altered; her pulse was sixty, countenance pale, belly costive, catamenia regular. I directed successively, laudanum, æther, bitters, the saline julep, and Schweppe's Seltzer water; I also ordered a warm plaster to be applied to the stomach, and some aloetic pills to obviate costiveness. I advised that she should take her food in small quantities, and use gentle exercise on horseback, which she had previously tried with some apparent benefit. Her retaining the whey at night, suggested the idea of substituting broth, which answered for some nights, but was afterwards rejected as soon as taken.

Her friends perceiving her evidently growing worse under this treatment, brought her to Cork, in May last, that she might be under my immediate care, and have the advantage of additional advice. She was now much emaciated, her pulse was weaker, and her general strength very much diminished; the pain of her back and stomach was increased; her legs swelled towards evening; she still rejected her food and drink without any apparent effort; she had not above two hours sleep at night, from which she awoke unrefreshed. At this time my friend, Dr. Sagrue, saw her with me, and suspecting an affection of the liver, suggested the trial of mercury, which was used so as to excite slight ptyalism without any advantage. A blister to the stomach, and another to the back soon after, intirely removed the pain. Some internal remedies were given of the same nature with those already mentioned; but perceiving

ing an undoubted fact, that many of the delicate young ladies of the town of Macromp, where Miss M'Sweeny resides, actually live on one meal a day, during the entire of Lent annually; and as meat is prohibited, and they seldom have an opportunity of getting fish, this meal is in general nothing more than bread and butter and tea. The consequences may be easily imagined. It is to be hoped that a more liberal policy, with regard to the clergy of this extensive sect, will shortly put an end to this profligate waste of health and life.

ing no benefit from them, ſhe was confined to port wine highly ſpiced, and broth, of which, by the moſt cautious management, ſhe was at length able to retain about four or five tea ſpoonsful of each in a day. But, whenever we attempted to exceed this quantity, the intire of what ſhe had taken for the day was inſtantly thrown up. She ſoon after returned to the country, without leaving her friends or her phyſicians the ſmalleſt hope of her recovery.

Having occaſion, in a ſhort time after, to viſit a patient in her neighbourhood, I called on her, and was really ſhocked at the ſudden alteration in her appearance. She was in every reſpect worſe, the weakneſs and emaciation were increaſed, her features were collapſed, and her ſkin was of a deadly paleneſs; the period had paſſed without the menſes appearing; ſhe had ſcarcely any ſleep; ſhe conſtantly vomited at night the whole of the broth and wine ſhe took in the courſe of the day. I had juſt read the extracts from Dr. Marcard's work, (Ueber den Gebrauch der baden) in Dr. Beddoes's valuable Eſſay on Conſumption, which though they did not exhibit any inſtance exactly ſimilar to that of my patient, yet furniſhed ſome analogical facts, not unfavourable to the trial of the warm bath in her caſe. From the manner in which the diſeaſe originated, and the extreme debility which it had induced, I derived ſome faint hopes from the gently ſtimulating effects of this remedy. The firſt trial, however, was rather diſcouraging. The bath was heated to about 98 degrees of Fahrenheit. She was ſeized with a vomiting the moment ſhe entered it, which ceaſed on her being taken out. She thought ſhe felt weaker the remainder of the day, and had frequent returns of coldneſs and ſhivering. Next day, the bath was made ſome degrees warmer, and ſhe continued in it a quarter of an hour, without the leaſt inconvenience. She felt warm and comfortable the intire day after. She retained a quantity of milk and tea on her ſtomach, and ſlept ſix hours that night. Encouraged by this trial, ſhe perſevered in the daily uſe of the bath, gradually increaſing the temperature of it, with the ſame beneficial effects. She ſoon gained ſufficient ſtrength to bear the exerciſe of the ſwing, which ſhe had before attempted, without ſucceſs. Her diet was milk, broth, jelly, tea, and coffee; and by my advice ſhe ſoon after added, bread, potatoes, cauliflowers, and artichokes, without any return of the vomiting. But ſhe found, on repeated trials, that meat in a ſolid form, however prepared, or ſeaſoned, would not remain a moment on her ſtomach. In about ſix weeks ſhe was ſo much recovered, as to think the bath no longer neceſſary, and by my advice ſhe diſcontinued it. I ſuſpect that rejection of meat in a ſolid form, depends rather

rather on ſome nauſeating aſſociation, than on any remaining debility of the ſtomach, as ſhe retains and digeſts ſubſtances, which are in general conſidered as much more difficult of digeſtion. She has lately made no trials with it, and yet her health is perfectly reſtored, and ſhe is even fatter and more blooming than ever.

I lament that the degree of the thermometer to which the bath was heated, was not daily aſcertained. It muſt have been greater than the ordinary temperature of warm baths, as her ſiſter aſſured me, that it was in general ſo hot, that *ſhe* could not bear her hands in it.

The above ſtatement was drawn up at different periods, previous to October or November laſt. I ſaw Miſs M'Sweeny a few days ago; ſhe continues well, and very much diſpoſed to fat; ſhe began to eat meat in laſt month, and has continued it ſince without any inconvenience.

There is another doctrine, which appears to be better founded, that has been objected againſt the warm bath, viz. the danger of catching cold. A medical friend of mine, who unites an ardent love of improvement with the experience of a long life, has been many years in the habit of recommending the warm bath in caſes of great exhauſtion from lues venerea, as well as to obviate the debilitating effects, occaſioned by the immoderate exhibition of mercury, with the happieſt ſucceſs. He has aſſured me, that in ſuch caſes he has never found it neceſſary to injoin any precautions for avoiding cold. On the contrary, when the patient's ſtrengh was ſufficient, he always adviſed him to go into the air after bathing, and never obſerved any bad effects from it, even in cold weather. The luminous theory of Brown, ſo happily explained and illuſtrated by Dr. Beddoes, deſerves attention on this ſubject.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

THE early and polite attention which you did me the honour to pay a former Eſſay, emboldens me to ſubmit the following remarks to your conſideration. I am,

GENTLEMEN,
With great reſpect,
Your very humble Servant,
S. C. BLYTH.

Army Hoſpital, Yarmouth,
July 14, 1800.

On the prevailing Taste for Hypotheses in Medicine, with a Review of ancient Practice in acute Diseases.

IT is somewhat strange, that though in many arts and sciences, improvement has advanced in a step of regular progression from the first moment of their invention, in others it has kept no pace with time, and we look back to ancient excellence with wonder, not unmixed with awe.

Medicine seems to be one of those ill-fated arts, whose improvement bears no proportion to its antiquity; for it is lamentably true, that although Anatomy has been better illustrated, the Materia Medica enlarged, and Chemistry almost created since the revival of letters, we are very little before the ancients in the penetration which discovers the precise nature of a disorder, or in skill to apply the remedy.

Perhaps it may be on account of those very advantages, that modern practitioners are inferior to those of ancient times. We have so many collateral helps, that we seldom study Nature in her original character, but see her at second hand, often deformed by the injudicious narrator, or disguised and fashioned by the plastic hand of the Theorist, to suit a favourite hypothesis.

Hippocrates, the father of his art, and the prince of physicians, has said, that a physician should be the minister of Nature; and is it not our departing from Nature, in compliment to new theories, that modern practice appears so diminutive, compared to that of the illustrious native of Cos? A review of his mode of treating acute disorders may justify these remarks.

The celebrated Grecian's first principle was, "That the end of physic is either to carry off diseases, or to moderate their violence."—Accordingly, his indications in acute diseases were "to mitigate the symptoms, when too violent, and to assist Nature in her business of concoction or evacuation of the febrile matter."—Having taken these indications from Nature, when the fever was irregular, and had no certain type, he waited without doing any thing till it became regular, and then he proceeded according to the predominancy of the symptoms.

Considering a fever in every circumstance as an effort of Nature to rid herself of a load, he never attempted *to cure it*,—that is, he never wished to arrest the febrile commotion altogether.—He restrained these efforts by bleeding in the beginning of acute disorders; but not to promote an artificial crisis,

crisis, (as has been imagined) or otherwise he would have bled on the critical days, and not in the beginning of a fever.

Another method Hippocrates took to moderate the violence of acute diseases, particularly pleurisies, peripneumonies, and ardent fevers, was by giving cooling and emollient clysters. In pneumonic complaints he limits the exhibition of these to the first four or five days, as no evacuation was to be excited after the patient began to expectorate, lest he should die of a suffocation. To this general rule, however, he instances exceptions in his own practice.—The third object was to regulate the patient's diet.

For this purpose he made use of two ptissans:—Pearl barley dissolved in ten or fifteen parts of water was called ptissana tota, or whole ptissan; when this was strained, and the thinner separated from the thicker parts, it was called ptissana colata, or the juice of ptissan. His rules for giving it were these:—the more acute the disease, the more thin and watery the diet; and it ought to be thinnest about the height of the disorder. In ardent fevers he seldom gave ptissan till after the crisis; and he directs, in one passage, not to give it till there are signs of concoction in the urine. But, in fevers attended with more moderate symptoms, he directs whole ptissan to be given from the first access, to support the patient's strength. Throughout the whole course of the fever, he directs diluting liquors to be given in the greatest abundance. To promote expectoration in pleurisies and peripneumonies, he orders the strained ptissan to be given mixed with honey, and directs, when the matter begins to be expectorated, to apply warm fomentations and liniments externally, to promote a maturation of it. Sweats he considered as a critical evacuation when they appeared on the critical days; he therefore recommended artificial means to procure them; as, warm bathing, diluting freely, and sometimes anointing the patient with oil. With a view to carry off the febrile or peccant matter, Hippocrates ordered purges at the beginning and end of acute disorders; he deferred it, however, generally till the fourth day, that he might learn more distinctly the nature of the fever. In diseases strictly inflammatory, he abstained from purging while the urine was pale and crude, a sign that the febrile matter was fixed. He was against all evacuations during the middle stage of acute disorders; and says, "that as the symptoms are most violent about the height of a disease, we should rather assist Nature in the struggle, than weaken her by any evacuations at that time;"—this, however, applies to continual fevers, for he purged in a tertian on the eighth day.—With respect to purging at the conclusion of fevers, he directs, that when they terminate

terminate by resolution, or a simple concoction of the febrile matter, and thereby changed into a healthy state, no purge should be given afterward. But if there has been a critical concoction or evacuation, he directs a purge for fear of a relapse *from an imperfect crisis.*

Such was the practice of this admirable physician. He calls his art "the most excellent of all arts," and it lost not its value in the hands of such an artist.—Penetrating and indefatigable, he studied Nature closely. No symptom, no indication escaped him; and by restricting his practice to the suggestions of the complaint, he anticipated their approach, and seldom failed to give relief.

His doctrine, however, though it stood its ground for four hundred years, was found too simple for the profound speculators of after-times; and Asclepiades rejected it and set up another, which happily did not last long, but was overturned by Themison, who cured all diseases by bleeding, purging, and cold water. Theories now multiplied fast, and new systems of medicine appeared every day. At length came Galen, who seemed born for the restoration of the Hippocratic practice. In acute diseases particularly, he never failed to tread in the steps of his illustrious predecessor, whether with regard to assisting or restraining Nature; purging at the access and decline of fevers, ordering expectorating medicines, after concoction, or any other dicta of so eminent an example.

After the revival of the Hippocratic practice by Galen, it stood firm for many centuries; but it was once more destined to sink under opposition. Argentarius, Pereira, and Fernelius began the attack on the Galenic mode, which gradually declined till Paracelsus and Van Helmont gave it a fatal blow. After a second eclipse of its merit it has partially revived, within the last century and a half, under the fostering hands of Sydenham and Boerhaave, who incorporated the ancient practice with their own, and who, we may safely add, owe more of their reputation to the diligent study and imitation of Hippocrates, than to any other cause.

A popular writer observes, that "the difference between a good physician and a bad one, is very great; but the difference between a good physician and *none at all*, is oftentimes very little. Hippocrates was a medical centinel; he was all eye, ear, and vigilance. Every thing relative to a complaint was minutely attended to; and with a dexterity of inference, unknown to modern practitioners, he knew what was to be done, and the precise moment for doing it.

Theories are but the butterflies of the day—they buzz for a while, and then expire. We can trace, for many centuries past,

paſt, one theory overturning another, yet each in ſucceſſion promiſing itſelf immortality. Who imagined that the doctrine of phlogiſton was to be overturned ſo ſoon?—In fifty years more it may be forgotten.

But the practice of Hippocrates will endure for ever; for it is founded in truth, whoſe immutability neither time nor accident can control.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

WITH much pleaſure I embrace the earlieſt opportunity to announce to the medical world, through the channel of your Journal, that I have ſucceeded in the cure of ſcurvy by the Citric Acid, in the *concrete form*, as prepared by Mr. Coxwell of Temple Bar, after the manner of Scheele.*

The hiſtory of health in the fleet for the laſt ſeven years has afforded us ſo large a ſtock of facts, that every information ſeems to have been obtained that could be wiſhed, for the effectual prevention and cure of ſcurvy; but there was ſtill a *deſideratum.* Some preparation of lemon juice was wanted, on a large ſcale, that could be preſerved in every climate for the longeſt voyage, and that ſecured all the antiſcorbutic virtues of this vegetable acid. Mr. Coxwell's preparation, in the form of cryſtals, poſſeſſes all theſe qualities: I alſo underſtand, that he can ſell it at a price lower than the common juice, as now ſupplied to the King's ſhips; and I can vouch, that its powers againſt this ſea diſeaſe, are equal to *any effect I have ever obſerved from the recent fruit in its moſt perfect ſtate.*

It is highly gratifying to myſelf, to find that the navy and commercial part of the country are now furniſhed with the laſt article that is neceſſary to preſerve the lives of ſeamen againſt a diſeaſe that is inſeparable from long voyages. Let me, however, remind the officer, the navigator, and merchant, that the crews of ſhips ought, nevertheleſs, to be recruited, at as ſhort intervals as poſſible, with freſh meat and eſculent vegetables. I ſhould always like to ſee the lemon, in all its conditions, rather truſted in the cure, when we can do no

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* Vide Crell's Journal for 1784.

no better. A long uſe of it weakens the digeſtive powers, and impairs muſcular action; and thus adds another cauſe to the liſt of evils to which ſeamen are expoſed, in bringing on that premature old age ſo generally among them.

It takes from ſixteen to eighteen parts of water to bring Mr. Coxwell's acid to the ſtandard of lemon juice. The gums of the patient never bled after its firſt adminiſtration; they became florid on the third day; the colour of large livid blotches on the third day, alſo, were evidently different in their hue, and ſoft to the touch: other ſymptoms declined in the ſame proportion; and all this during the uſe of the ſhip's common fare of ſalt meat, &c. in order to ſhow its efficacy in the moſt forlorn ſtate, and where the diſeaſe was well advanced. We ſhall, on ſome future occaſion, detail the firſt caſes; where it will be obſerved, that we have carefully avoided thoſe errors into which others have fallen, on holding up new remedies againſt ſcurvy.

The mind of the philoſopher will naturally offer ſpeculations on this intereſting ſubject; that an acid, of vegetable origin, after undergoing the proceſs of ebullition, combination with a calcareous baſe, and, at laſt, diſplaced by the ſuperior attraction of another acid, ſhould retain the ſame effect on animal life, which it had in its recent form. But, "*we are fearfully and wonderfully made*, and here all conjecture muſt ſtop.

The medical readers of your Journal, who are connected with the mercantile world, will naturally extend this information. In the Eaſt India ſervice, and the Southern Whale Fiſhery, ſcurvy is a frequent diſeaſe. The concentrated ſtate of this preparation of lemon juice, reduces the ſhip load to the ſize of a portmanteau. A beſieged garriſon might be relieved by dropping from a balloon a few pounds of it, when ſcurvy rendered it untenable, as happened with Fort St. Philip, at Minorca, laſt war. I am,

GENTLEMEN,

Your moſt obedient humble ſervant,

T. TROTTER.

Plymouth Dock,
July 13, 1800.

P. S. Since finiſhing this letter, I have been favoured with the following remark, by one of the reſpectable and able ſurgeons who has been engaged in this inquiry. He ſays, "I muſt now confeſs, that I have been not a little aſtoniſhed at the effects of the *concrete acid*, they have far ſurpaſſed my moſt ſanguine expectations: the proportion of an ounce of *concrete ſalt* of lemon to a pint of water, as uſed by me, taſtes ſtronger of the *citric acid* than the common lemon

juice.

juice. It is much more agreeable and palatable, being without any of that mufty tafte which the lemon juice always acquires."

His Majefty's Ship, Superb, T. WATHERSTON.
July 12, 1800.

Mr. Watherfton alfo fays, that cafes of fcurvy, though with milder fymptoms, taking the common lemon juice from the fame date, continue on his lift, while the others are returned to duty.

To the Editors of the Medical and Phyfical Journal.

GENTLEMEN,

IN a late Number of your Journal* appeared a paper from Dr. Squire, containing ftrong and pointed animadverfions on a doctrine detailed in a former part of your publication† by Mr. Peck, recommending the expediency of an early extraction of the placenta. The opinions in queftion, on this fubject, differ extremely; and on no occafion, perhaps, could the almoft univerfally applicable advice of the father of Phaethon, " *Medio tutiffimus ibis*," be more advantageoufly regarded than in the prefent inftance.

While an implicit compliance with the admonition of uniformly proceeding to extract the placenta by manual operation, if retained longer than fifteen minutes after the expulfion of the child, though neither hæmorrhage nor any other urgent circumftance fhould indicate its neceffity, might, in general, be deemed an undue anticipation of the feafonable efforts of uterine power, and, in fome cafes, actually prove pernicioufly precipitate; a rigid conformity to the precept inculcating indefinite delay, if no hæmorrhage occur, would lead to a practice invariably pufillanimous, and, occafionally, more precarious and difaftrous, than can be fairly charged to the account of the other extreme.

The worft effect likely to refult from a premature but well conducted extraction of the placenta is, aggravation of pain during the operation; the probability of fubfequent evil can fcarcely be an object of dread, as the ftimulus of *vacuity* on the extrufion of the placenta, is generally fufficient to excite the

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* Vide No. XV. p. 459. † Vide No. XIII. p. 221.

the uterus to an approximation of its parietes, which, at once, effectually obviates hæmorrhage, and appeases inordinate irritation. The mischief that may accrue from indeterminate delay, consists in the depressing anxiety which it is liable to induce in the patient; the debilitating continuance of inadequate uterine irritation; incidental hæmorrhage from partial detachment of the placenta, and the alarm inevitably occasioned by being eventually compelled to perform under the distress of urgent necessity, what might have been more seasonably executed with the composure and address inspired by prudent precaution.

My opinion on this controversial point, has arisen solely from long and extensive experience, and fully warrants me in adopting an intermediate course of practice.

Directing my conduct not by any abstract rule, but exclusively by the circumstances of the case; after the birth of the child, and as soon as the divided extremities of the funis have been secured by ligature, my operative attention is incessantly engaged in the removal of the placenta, neither by violence, nor in negligence of uterine effort; but, by keeping the funis moderately stretched, varying the line of draught from the perpendicular to an anterior, posterior, and lateral direction, cautiously co-operating with every contraction of the uterus, by a guarded increase of extension. In this manner, invariable success has attended my endeavour within half an hour; no untoward circumstance has ever occurred; the funis has in no instance been ruptured, nor that event even endangered, when it possessed its natural firmness; no hæmorrhage has been induced; no complaint of augumented pain, nor any sort of subsequent inconvenience. This practice is now so perfectly familiar to me, that I have the utmost confidence in its adoption; and feel equally justified in its pursuit, by principles of *science* and *humanity*. *Scientifically*, because it appears to afford Nature the peculiar aid which moderate tension only is adapted to render, in obviating the enfeebling flaccidity which occurs in a comparatively vacant state of the uterus after the exclusion of the child, and in at once gently exciting and reinforcing the natural disposition to uterine contraction. *Humanely*, because the exertion proposed can inflict no injury, and by insuring a definite expulsion, all anxiety is terminated, and contingent disaster precluded.

From the preceding statement, it will be inferred, that my practice has more analogy to that proposed and vindicated by the advocates for early extraction, than to that recommended and espoused by the partisans for unlimited delay; but, that while it differs from the former in superceding the necessity

for direct operative detachment of the placenta, (that ultimate occasion having never presented in my own practice, in the course of many hundred cases) it varies from the latter, in proceeding immediately to assist the uterus by moderate cooperation, in exonerating itself of what may be considered as a morbid state of excitement.

It has twice occurred to me to be called to assist gentlemen, who, adopting the doctrine of patiently awaiting the spontaneous exertions of the uterus for the expulsion of the placenta, had, in both instances, allowed it to remain upwards of twelve hours, during the whole of which time, inefficient pain had recurred at intervals. The mode of assistance herein proposed, was immediately employed; and, in both cases, within half an hour, the placenta was safely removed, without superinducing any sensible degree of additional irritation; nor did hæmorrhage or any other inconvenience supervene.

It is uncertain how long the placenta, in the instances alluded to, might have been retained; probably, until the inadequate contractions of the uterus had effected a partial detachment, and the consequent hæmorrhage had compelled the *patient* practitioners to furnish manual aid. The mental suffering of the patients would have been cruelly aggravated by longer delay, and no compensation could have accrued from superior advantages, by ultimate natural expulsion.

Dr. Squire has adduced a formidable legion of authorities in support of his opinion,* and appears, very consistently, to have assumed no small portion of confidence from the respectable mass of suffrage he has cited. It would be equally to calumniate Nature and the defenders of her competency, to declare, that artificial aid is *uniformly* necessary in placental delivery. In fact, it is but very rarely, that art need be resorted to, either to avert or remedy error. Indeed, the mere possibility of accidental deviations from natural rectitude, affords the only justification that can be assumed for the ordinary practice of midwifery. It not unfrequently occurs, that women, even in civilized nations, safely undergo parturition in solitude; nor has the obstetric art been familiarly known in countries where vicious refinements have not perverted the order of Nature, and introduced the various physical and moral evils of licentious civilization.

As the practice of midwifery is not uniformly necessary, and only occasionally required to countervail constitutional degeneracy and mal-conformation, chiefly originating from the corrup-

* Vide Medical and Physical Journal, No. XV. pp. 482—3.

rupt inſtitutions and habits of art; it is peculiarly important, ſtrictly to regard the legitimate objects of aſſiſtance, that ſuitable aid may be correctly and promptly rendered when indiſpenſably requiſite; and not be uſeleſſly, or pernicioully obtruded, on what is excluſively the work of natural power.

The ordinary progreſs of parturition, under natural preſentation, cannot be advantageouſly aſſiſted by art; due patience will ultimately evince the fidelity of Nature to her engagement.

The ſupport of the perineum is a precaution more worthy of the kind office of a kindred female, than of the ſolemn parade of obſtetric ſkill. The benefit afforded in ſecuring againſt its laceration, by moderating and regulating the uterine impulſe, is imaginary. Scarcely a calculable probability exiſts of that event occurring; and the reſiſtance uſually oppoſed to it, could not prevent the accident, while it may injure the uterus by counteracting its protruſive efforts. The expulſion of the placenta ſafely admits of definite acceleration; and, in natural parturition, its ſeaſonable removal may be truly ſaid to conſtitute the ſole utility of midwifery practice.

In a large majority of caſes the placenta would, no doubt, be ſpontaneouſly extruded; but it will not unfrequently happen that much anxious protraction of time, alarming hæmorrhage, and unavailing irritation, may be happily prevented by its early removal.

The mode propoſed, approaches to an imitation of Nature in ſuſtaining that tenſive ſtate of the uterus which preceded the expulſion of the child, and which would be continued in ſome degree after birth by the infantile ſtruggles, were the funis to remain unſevered.

A firm perſuaſion, founded on ample *experience*, of the perfect ſafety and expediency of this practice, is my only motive for ſubmitting it to public conſideration. It need be but tried to be approved, and its uniform adoption would end all ambiguity and poſſible hazard relating to *placental deilvery.* I am,

GENTLEMEN,

Your's, reſpectfully,

ROBT. KINGLAKE.

Chilton ſuper Poldon,
July 12, 1800.

To the Editors of the Medical and Physical Journal.

Gentlemen,

As the professed design of your valuable work, is to constitute a vehicle of useful information in the various departments of physic, for the profit and entertainment of your numerous readers; permit me, through the same channel, to suggest a few thoughts, with all due deference, on a subject which I do not recollect to have been hinted at, in any former number of your miscellany.

The subject on which I purpose to offer a few remarks, is, notwithstanding it may to some appear trivial, found to be of the utmost importance in medical practice, viz. *on the necessity and advantage of gradually augmenting the dose of active remedies in the treatment of obstinate chronic diesases:* An advantage, no doubt, well known to the more sagacious and experienced part of the profession, but, perhaps, too much neglected by the raw and injudicious; in consequence of which, I apprehend, that not unfrequently much credit is lost on the part of the practitioner, as well as benefit on that of the patient.

It scarcely needs to be observed, that that sensative organ, the stomach, through the medium of which the influence of medicines is conveyed to the whole system, loses, in a great measure, the stimulus of the medicine applied, after it has been accustomed to it for a given time; and hence a reinforcement becomes inevitably necessary, by an augmentation of the dose, in proportion as it is capable of receiving it, by which means the curative plan is prosecuted with considerable advantage, and the symptoms of the disease are most likely to be subdued.

In no instance, perhaps, is this observation more fully confirmed, than in the treatment of *chlorotic patients*, where a long course of steel, in conjunction with tonic bitters, is indispensably necessary. In this case, the subjects are for the most part in a state of great debility, the stomach extremely irritable, and ill disposed, at first, to retain the nauseating quality of the *ferrum*, in any form in which it can be administered, and in such a dose as is requisite to produce the desired effect. Therefore, by commencing the course with small doses, and gradually increasing the quantity during the treatment, we find the stomach reconciled to it, and the patient seldom complains of nausea or sickness; but, on the other hand, derives every benefit, generally speaking, by this mode of its administration, which it is expected to communicate to the system.

The same observation will, it is presumed, hold good, as it

respects the administration of other active and important remedies; such as, *hydrargyrum*, *cinchona*, *opium*, *cicuta*, *scilla*, &c. where the cases demand a long continuance of their use. Nor is the hint conveyed, as containing in it any thing new, or not already known, but generally too much neglected; for the writer is well aware of the highly improved state of physic in the present day, and of the numerous practitioners who are justly deemed an honour to the profession. Therefore he flatters himself, that the idea he has suggested will not be construed *by them* as any indirect reflection on their superior skill, but as an humble attempt to serve the general cause, and to promote the good of those, who place an unlimited confidence in, and anxiously expect to receive benefit from, their medical attendants. I am,

GENTLEMEN,

Your's, respectfully,

H. DAVIES.

Piccadilly,
July 9, 1800.

On the Medical Use of Phosphorus.

PHOSPHORUS, whose use was introduced into medicine about fifty years ago, is known as the most powerful exciting remedy which we possess. Several physicians have tried it, with more or less success, in all cases where a high degree of nervous debility prevails; and accordingly, in nervous fevers, palsies, impotentia virilis, epilepsies, &c. also in arthritis nodosa et atonica, it has proved very efficacious; and it likewise deserves to be used in cases of persons poisoned with lead or arsenic. There exists, at least, an instance of a person, who having been given poison in Italy, (probably aqua toffana,) became quite emaciated, his nails and hair fell off spontaneously, and his life was only saved by phosphorous. Its effects are at the same time in a considerable degree diuretic and diaphoretic.

Experience has, however, shown that the application of this remedy requires the utmost caution, and it may sometimes prove very dangerous if proper care is not taken; as it has been observed, either to cause a vehement inflammation in the stomach and bowels, accompanied with very burning pains in the regio hypochondriaca, and often terminating in death, or by producing indurations, or scirrhus of the stomach, which occasion chronic cardialgies, indigestions, an habitual vomition of whatever is taken, emaciation, and hectic death. These ill consequences

consequences are in a great measure owing to the mode in which it is administered; either by giving it in substance, or in too great a dose; as, in the first case particularly, the minutest particle of it may, by adhering to the stomach, produce an inflammation, or at least an induration.

As for the dose, it has been observed, that it never must exceed two grains in 24 hours, and a burning pain or sensation was always perceived whenever it was given in a greater quantity, but generally one grain in 24 hours will be sufficient to produce the desired effects.

The form in which it is to be exhibited ought to possess the quality of containing the phosphorus quite dissolved, and at the same time of obtunding its too dangerous irritation upon the stomach. The forms hitherto used were a mixture with a conserve, which is of all others the most improper, because the remedy is applied here in substance, and may of course easily bring on the above mentioned symptoms; a solution of it in oil, which, though otherwise very proper, is too disagreeable to take; a solution in ether, or naphta vitrioli, in which the phosphorus entirely dissolves; but, by this combination, its heating and irritating qualities are exceedingly increased; and as only eight grains of phosphorus are soluble in one ounce of the ether, 120 drops must be taken, only to bring one grain of it into the body, which in most cases would be too dangerous. However, this naphta phosphorata may be of great benefit or use in a very high degree of nervous debility, combined with a total want of sensation.

The following form of giving phosphorus internally, will therefore be the most proper, on account of containing it dissolved and equally divided throughout, and having it sufficiently involved in mucilage, to prevent any ill consequences from a local irritation of the stomach even in very sensible constitutions. The phosphorus is rubbed with sweet almonds, or a sufficient quantity of gum arabic, to make an emulsion, and by an addition of spiritus nitri dulcis, or liquor anodynus, the taste and smell of the phosphorus is rendered less disagreeable.—

℞. *Phosphor. urinæ gr. ii. subigantur longa trituratione cum mucilagine gr. arabici q. s. ut fiat cum aquæ fontan unc. vj. emulsio, cui adde syrupe de althaea unc. j. liquor. anodyn. miner. Hoffmann. gutt. xxx.* D. S. *omni bihorio cochlear sumendum aut plus. pro re nata.* In this way a very efficacious solution will be obtained, which shines in the dark, and is agreeable enough to be taken. (*From Hufeland's Practicul Journal, vol.* vii. *No.* 3, 1799.)

Of the external use of phosphorus, particularly in caries ossium, we shall, perhaps, say something in one of the future numbers of this Journal.

CRITICAL

CRITICAL RETROSPECT OF MEDICAL AND PHYSICAL LITERATURE.

[FOREIGN AND DOMESTIC.]

Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge.

[Continued from our last Number, pp. 67—71.]

WE renew our Analysis of this Volume with great pleasure, as it contains several important papers; and we select the following, because it furnishes a very useful hint, and from which much advantage may be derived in the practice of midwifery.

" *Observations on the Management of Cases in which the Face of the Child presents towards the Os Pubis, by* JOHN CLARKE, M.D.

" Every person who has been engaged in the practice of midwifery knows, that if in labour the face of the child lies towards the symphysis pubis, considerable difficulty is thereby frequently occasioned.

" More force must be exerted by the woman, in order to expel the head so situated, and the labour will be protracted to a far greater length, than when the head is in the usual position, with the occiput towards the os pubis.

" The ground of this difficulty is, because in this situation, the whole of the face must descend through the pelvis, before any part of the head can emerge from under the symphysis pubis, and because the bones of the face are incapable of undergoing any alteration from pressure.

" On the other hand, when the occiput lies towards the os pubis, the different bones, of which the posterior part of the head is composed, are capable, in consequence of the incomplete ossification at the sutures, of very great alteration in their relative situation, so that the form of the whole head may become materially changed, and be better adapted to pass out of the pelvis.

" Thus the same head, from this variety in its position, may offer a very different degree of resistance to the powers of expulsion.

" If the head should, unfortunately, be placed with the forehead towards the os pubis, and the woman should be strong, her exertions and pains must be more violent in proportion to the increased resistance: but the labour, though prolonged very much beyond the ordinary bounds, may at length be finished by her own efforts. But in other instances the difficulty may be so considerable, that her strength

ſtrength may be worn out without accompliſhing the birth of the child; and ſhe muſt either remain undelivered, or artificial force muſt be ſubſtituted for the natural powers, which are found to be defective.

" This unfavourable poſition of the head may be detected by a very little attention to the ſituation of the anterior fontanelle, and of the ſutures.

" If, on examination, the anterior fontanelle be felt, and the ſagittal ſuture be found running from it towards one of the ſacro-illiac joints, or directly towards the concavity of the os ſacrum, there remains no doubt that the face will be born towards the ſymphyſis pubis.

All the beſt writers upon the practice of midwifery have taken notice of this cauſe of difficulty in labour; but they have been contented with deſcribing it, without ſuggeſting any means more eſpecially ſuited to this caſe. A reliance upon time, when the woman has ſtrength, and the application of inſtruments, when ſhe has not, comprehend all the practical advice which is contained in their works.

" Chance firſt led me to the knowledge of the fact, that, in ſome caſes, this poſition of the head can be remedied without ſubjecting the mother to any additional pain, or the child to any kind of danger.

" In a caſe where I had reaſon to expect ſome danger, I was deſirous of knowing the preciſe poſition of the child's head, and whether it was in a ſituation which would admit of delivering ſafely with the forceps, if this ſhould become indiſpenſably neceſſary. I found the face turned towards the groin, and on endeavouring to aſcertain whether the ear could be felt, I was obliged to make a firm preſſure againſt the ſide of the head with my finger.—In doing this it appeared to be moved a little. Aware of the great advantage which might ariſe to the patient if I could ſucceed in bringing the occiput to the pubis, whether ſhe were ultimately delivered by nature or by art, I continued to make preſſure upon the ſide of the head, till in the ſpace of a few minutes the occiput was brought to the groin from the ſacro-iliac joint of the ſame ſide; the conſequence of which was, that, inſtead of the face, the occiput was born towards the pubis, and thus conſiderable pain and difficulty were avoided.

" Reflecting upon the event of this caſe, I thought that the ready alteration in the poſition of the head might, in this inſtance, depend upon the pelvis of the woman being very large, relatively to the volume of the child's head, and that a ſimilar change could not be produced by the ſame means in other caſes of a ſimilar preſentation.—I determined, however, to make a trial in the next caſe which ſhould occur. Another caſe ſoon occurring, this practice was attended with equal ſucceſs.

" I have now met with fourteen caſes, in thirteen of which the practice has ſucceeded; and as ſome years have now elapſed ſince the firſt caſe, I think myſelf fully authoriſed in recommending this method

method to be always purſued, when the face is found in the ſituation above deſcribed. A great deal of pain, and much time, will be ſpared to the patient by theſe means.

" The manner of effecting the change is, by introducing one or two fingers between the ſide of the head, near the coronal ſuture, and the ſymphyſis pubis, and preſſing ſteadily againſt the parietal, or frontal bone, during a labour pain. When this is done, it will be found, in moſt caſes, that the head yields to the preſſure, till at length the occiput is brought to the groin. This being effected, the reſt ſhould be left to the natural efforts of the woman.

" It is unneceſſary to obſerve, that this alteration will be more eaſily produced, when the face lies towards the groin, than when the ſagittal ſuture runs directly backwards to the ſacrum: but even in this caſe, the change of poſition may be effected with much more facility than I before hand ſuppoſed to be poſſible

" When the pelvis happens to be large, or the head of the child ſmall, as there will be more room for the head to turn, the difficulty of doing it will become proportionably leſs.

" In ſome inſtances, where the pelvis is ſmall, or the head large, or where the Face is directly turned to the pubis, it may be impoſſible to change the direction: but theſe caſes are comparatively rare; and as no harm can ariſe from it, the attempt may always be made."

" *A Caſe of Pregnancy, in which the Ovum had become diſeaſed, and was entirely filled with ſmall Hydatids. By* EVERARD HOME, *Eſq.* F. R. S.

" THERE are many caſes mentioned of the placenta loſing its natural ſtructure, and becoming a congeries of Hydatids; ſeveral of theſe have been publiſhed: but in all the inſtances on record, as far as I am acquainted with them, the patient had miſcarried, and the placenta was examined after being ſeparated from the uterus, ſo that the diſeaſe could not be exactly aſcertained.

" The following caſe, in conſequence of its melancholy termination, afforded an opportunity of the parts being examined in their natural ſituation, and of determining preciſely the ſeat of the diſeaſe.

" Elizabeth Yeoman, thirty years of age, was delivered of her ninth child on the 22d of December, 1797, and recovered from her lying in in three weeks, as ſhe had uſually done after former labours. She continued in perfect health till the 1ſt of Auguſt, 1798, when a flooding came on, which was ſuppoſed to be an uncommon flow of menſtrual diſcharge; this continued for ten days; it created an alarm, and an accoucheur was conſulted.

" He found, upon enquiry, that ſhe had not menſtruated before for twelve weeks, and there was an evident enlargement of the abdomen; he had therefore no doubt of her being pregnant, and thought ſhe had advanced to the third month. From this explanation

nation of the case, the present hæmorrhage appeared to be a flooding preceding abortion.

"The ordinary means for restraining the flooding were used, and on the 14th it had entirely stopped. She was now attacked by a vomiting, attended by a hot skin, and a pulse at 109. These symptoms were relieved by a gentle emetic, and saline draughts with rhubarb.

"On the 20th, there was a return of the vomiting, with spasms over the whole abdomen, and the general bulk of the belly became much increased; these symptoms resisted all means used for their relief, and she died on the 22d, in the evening.

"Upon inspecting the body after death, the womb appeared to be of the size that it usually is five months after conception. The os tincæ was a little dilated, receiving with tolerable ease the end of the finger.

"A longitudinal incision was made through the anterior part of the uterus, extending from the os tincæ to the fundus; this incision was continued through the membranes of the ovum, but no cavity was found, no fœtus presented itself, and every part was occupied by an infinite number of hydatids of different sizes, from that of a pin's head to the size of a common grape, scarcely any of them being so large as a full sized grape. They were attached to the nearest surface, and to every part of the amnion equally.

"Upon separating the membranes composing the ovum from the uterus, which was easily done, the situation of the placenta was readily ascertained; it was near the os tincæ on the posterior surface of the uterus, and was unattached, a separation having taken place during the patient's life time; this had occasioned the flooding, which gave the first alarm respecting her safety. The surface next the uterus had not its usual shaggy appearance, but was nearly smooth; it scarcely projected beyond the membranes of the ovum, and was about two inches in diameter.

"The mass of hydatids which filled the ovum were all connected with one another; were very tender, and readily broke down when handled. This mass of hydatids was very different from those met with in the liver, which are spherical, and those with long necks and heads, found in the brain of sheep; they were connected to the surface of the membrane, and to those hydatids with which they were united, by short pedicles or necks. To ascertain whether there was any appearance of a fœtus, I examined the whole with minute attention, but could not perceive the smallest remains either of one, or of a funis; nor could I, on the inner surface, distinguish any thing like the ordinary structure of the placenta.

"The hydatid structure of the placenta, as a disease to which that part is liable, is, I believe, very well known, and specimens of it are preserved in different collections of anatomical preparations. My attention in this case was directed to the investigation of the disease; and, from the facts I have stated, it does not appear to be a change in the structure of the placenta, but a general affection

tion of the amnion. When this disease takes place, the natural healthy actions for the support of the fœtus are so much impeded, that its growth is arrested. This evidently happened in a case published with an elegant engraving of the placenta and fœtus, by Dr. Denman; and when the patient does not early miscarry, the fœtus disappears; and in all the instances where miscarriage has taken place in a more advanced stage of the disease, I believe no fœtus had been found."

The following is an excellent example of the effect of the application of Gastric Juice to sores.

"*On the Use of the Application of Gastric Juice to Sores. By* John Harness, M. D. *Physician to the Fleet.*

"Mr. Thomas Corben, boatswain of his Majesty's ship Egmont, was, on the 31st of July, 1796, received into the Dolphin hospital ship, in consequence of a very bad ulcer of the right leg, which was fully six inches in length and four in breadth. The surrounding integuments were detached for some considerable way, and their margin, with very much the greater portion of the surface of the ulcer, was in a sphacelated state. The matter discharged was so extremely acrid, as to excoriate every part which it touched, and had insinuated itself through the whole length of the interstices of the gastrocnemius and soleus muscles. From between these muscles, which were detached from the bone nearly the whole length of the ulcer, a very large quantity of a most offensive matter was discharged, a considerable portion of which lodged in a cyst formed by the detached integuments on the posterior and inferior parts of the leg.

"There was a very great tension the whole length of the extremity.—The general health was much impaired.

"Having, in the course of a long and extensive practice, been too frequently a witness to the inefficacy of the applications hitherto made use of in the navy in the treatment of scorbutic ulcers (viz. cytric acid, bark, myrrh, rhubarb, and opium, separately or variously combined) and more particularly in warm climates, where sphacelus and erosions have taken place in any considerable degree, to which every seaman is peculiarly liable; I was induced, from the very unfavourable appearance of the case above related, to have recourse, with the concurrence of Mr. Gray, surgeon of the hospital ship, to the *Gastric Fluid of graminivorus animals*, which I knew could be readily obtained from the bullocks and sheep killed daily for the use of the fleet. From a bullock killed in the evening, nearly three pints of the juice were obtained, with part of which the surface of the ulcer was washed, as well as all the sinuses, by injecting the fluid into them in different directions by a syringe, with a long and slender tube Superficial dressings of lint were then applied, and particular attention was paid to prevent, by compresses and bandages, a further insinuation of matter, as well as to

 preserve

preserve the parts in contact, as far as their diseased state would admit of, that every advantage might be derived from the adhesive inflammation which I expected the remedy to excite. The above means were pursued night and morning. On removing the dressings upon the third day, the whole of the sphacelated parts was thrown off, which exposed a large portion of the tibia in two different places, to which pledgets of lint dipped in the fluid, were applied, which not only appeared to prevent the injuries usually attendant on those cases, (viz. discolouration and exfoliation) but on the contrary, in the course of eight days, the parts thus exposed were entirely covered by new granulations, and at the expiration of fourteen days, all the soft parts were perfectly reunited, and the surface of the ulcer reduced to a sore of about two inches and a half in diameter, with its granulations small, and compact, and of a beautiful florid colour. It is at present perfectly healed, and the patient, although a very lusty man, enabled, by the assistance of an elastic bandage, to attend to the duties of the ship.

"After the three first dressings, the Gastric Fluid of sheep only was used, not being able, from the ship being at sea, to obtain more from bullocks. The Fluid, if possible, should be procured fresh every day.

"To remedy his general ill health, an aperient medicine was first given; after which he took half a drachm of bark in two ounces of the decoction every six hours, with directions for him to make use of, as many lemons and onions in the course of the day as his stomach would easily bear. It was astonishing to observe with what rapidity he regained his strength after the sphacelated parts were thrown off.

"I have since had the pleasure of finding the application of the Gastric Fluid succeed in more than a hundred cases of sphacelus, and the testimonies given of its peculiar good effects in these cases by Mr. Jones, surgeon of the Naval Hospital at Bastia, and by Messrs. Bead and Buck, two of his principal assistants, convince me of the importance of making its efficacy generally known.

"P. S. I have at present a patient in the Dolphin, a seaman belonging to the Barfleur, Vice-Admiral Waldegrave, who, from being confined to his bed by typhus fever, became excoriated in three different places, the whole of which terminated in extensive mortifications; the sloughs of which are entirely removed by the Gastric Fluid, and the patient sufficiently recovered to walk about.

Observations on Mr. Simmons's Detection, &c. &c. with a Defence of the Cæsarean Operation, derived from Authorities, &c. &c. A Description of the Female Pelvis; an Examination of Dr. Osborn's Opinions relative to Embryulcia; and an Account of the Method of Delivery by Embryotomy. Illustrated by numerous Engravings. By JOHN HULL, M. D. Member of the Corporation of Surgeons, and of the Physical Society of London; of the Natural History Society of Edinburgh; and Secretary of the Literary and Philosophical

Philosophical Society of Manchester. Pr. 9s. 6d. pp. 480. Bickerstaff and Clarke, London.

When men of science differ in opinion on subjects of the utmost importance to the medical profession, and to the world at large, and confine the debate closely to the matter in question, the public are always benefited by it, unexpected lights spring from the contest, or at any rate the question has a better chance of being clearly understood. But it becomes extremely painful to us, who are solicitous for the health of the public, and the promotion of science, to observe two gentlemen of acknowledged ingenuity "*throwing dirt at each other*" with so little good resulting from it; and we plainly perceive, if this quarrel *descends* a very little lower, who *must* decide "*when Doctors disagree.*"

Dr. Hull appears to have been at infinite pains in collecting materials for his book, and has furnished the reader with many particulars, which will gratify his curiosity; however, the Author's design will be best explained in his own words.

"From the second part both information and instruction will, I hope, be derived. It contains the substance of a work announced for publication, under the title of a Treatise on the Cesarean Operation, previously to Mr. Simmons's unprovoked attack upon me. In this part I have criticised a very curious letter, addressed by Mr. S. to the Editors of the Medical and Physical Journal.—I have given a brief historical account of the Section of the Symphysis Pubis, and of the Cesarean Section, as practised upon the dead and living female, with tables of all the successful and unsuccessful foreign cases, which I have been able to collect.—I have pointed out the circumstances, rendering the operation necessary; and have laid down directions for accomplishing the delivery under circumstances supposed to require the performance of the Cesarean operation; but in which it is neither necessary nor admissible.—I have entered into an enquiry concerning the most proper time for having recourse to the operation; the best method of performing it; the sources of danger; and the subsequent treatment of the patient.—I have endeavoured to make a fair estimate of the comparative value of the life of the fœtus in utero, and the mother, according to the different conditions of the latter at the time of parturition. I have examined the opinions and precepts of Dr. Osborn, relating to hysterotomy and embryulcia; I have shewn both by reasoning and actual experiments, that they are ill founded; and I have attempted to lay down rules and directions, that are less exceptionable.—I have entered into a discussion of the propriety and advantages of inducing premature labour, with the view of superceding the necessity of embryulcia and hysterotomy. —And I have introduced a sketch of the female pelvis in its natural state, with an account of the distinctive characters of its deformities, and an enumeration of the causes, by which these are induced, and of the means, by which their different degrees may be most accurately determined."

The comparative estimate of the value of the lives of the mother

ther and fœtus, has been amply discussed by Dr. Osborn, in his Essay on Laborious Parturition. In these Observations, Dr. Hull has examined the subject with considerable ability, and, we think, has justly set a higher value on the life of the infant than Dr. Osborn, although he has not weakened (nor do we believe he means to lessen) our consideration for that of the mother. The learned author next proceeds to treat on the cases that may require the Cesarean operation; and he says, "It may be stated in general terms, that every circumstance *which can render delivery per naturales vias impracticable,* ought to be considered as requiring the Cesarean operation. What these circumstances are, writers are not agreed. For, whilst some authors contend, that no case, except an extreme degree of deformity and contraction of the pelvis can render the operation necessary, others enumerate a great variety of cases, as indicating it.

"The cases which I propose to examine, are: 1. Deformity of the pelvis. 2. Wounds of the *uterus*. 3. Rupture of the *uterus* or *vagina*. 4. Extra-uterine conceptions. 5. Obliquity of the *uterus*. 6. *Hernia* of the *uterus*. 7. Tumours, &c. affecting the *os uteri* and *vagina*. 8. Strange or foreign bodies diminishing the capacity of the pelvis. 9. Convulsions or Epilepsy. 10. Preternatural position of the *fœtus*. And 11. Monstrous conformation of the *fœtus*.—Of these I shall consider the first more particularly, and the others only in a general way".

On all these topics the author has taken a full view, and illustrated his arguments with several plates, most of which are well executed.

As much stress in the whole of the debate has been laid on the case of Elizabeth Sherwood, related by Dr. Osborn in his Essay on Laborous Parturition, we quote the following passage, which states the *degree* of difficulty in that particular instance in its proper light.

"Whether in any of the cases, wherein the Cesarean section has been had recourse to in Great Britain, the delivery could have been accomplished by any means with more safety to the mother, I am not able to determine; because we are not in possession of the dimensions of the pelvis in all of them. But, in every instance, where the dimensions have been given, the degree of contraction was greater than in the pelvis of Elizabeth Sherwood, if this be determined by the size of the largest circle that can be formed in the superior aperture, which seems to me to be the most proper mode of ascertaining their capacity. In the superior aperture of Sherwood's pelvis a circle may be formed equal to 1¼ inch; but we do not know that in any of the pelves, belonging to the poor women, who have undergone the Cesarean operation in Great Britain, a circle of the same size can be formed. We know that in the pelvis, of which I have just given a description, the largest circle, that can be formed in any part of the superior aperture, does not exceed one inch. It would therefore be by no means fair to infer, because Dr. Osborn asserts that he has delivered a woman by the crotchet, without destroying her, whose pelvis at the superior

rior aperture, would admit of the formation of a circle with a diameter of 1¼ inches, that he can deliver a woman, with safety, by the same instrument, whose pelvis, in the widest part of the superior aperture, will not permit the formation of a circle, the diameter of which is greater than one inch."

The author has collected with considerable industry a very full account of all the cases of Cesarean operation, and has reduced them into synoptical tables, with their fortunate or unsuccessful terminations.

"The first of these tables comprehends a short account of 110 cases of the performance of the Cesarean operation, and the third synoptical table, given in the 69th page of my Defence, &c. contains two, which, added together, make 112 successful cases. The second table contains 27 unsuccessful cases, which, with the 15 cases, contained in the first and second synoptical tables, given in my Defence, &c. p. 66 and 67, and the case, which has since occurred in the Lying-in Hospital at Manchester, make 43 cases, wherein the mother has died after the Cesarean section. Now, if we subtract the number of the unsuccessful from that of the successful cases, we shall find, that the latter exceed the former by 69."

For the Author's observations on the mode of performing the Cesarean operation, we must defer our account till a future Number of our Journal.

Some Observations on Vaccination, or the Inoculated Cow-Pox; by Richard Dunning, Surgeon, Plymouth Dock, and Member of the Medical Society of that Town and Plymouth, pp. 122. London, Cadell and Davies.

The ingenious Author is a strenuous advocate for the new inoculation, and appears to have taken considerable pains in investigating its nature and mode of action; and justly observes, that "we may inoculate the ladies with the utmost safety in the advanced stages of pregnancy; and from another of its distinguished traits—absence of contagion—nurses can be inoculated without danger of communicating disease to the infant, or. vice versa, the infant may be inoculated without injury, or subjecting the nurse to any indisposition or inconvenience whatever."

Critical Survey of the latest Theories on difficult Dentition in Children.

[Continued from pp. 77—82 of our last.]

We proceed now to give:

III. Dr. Wichmann's Theory, contained in his Ideas on Diagnostick, Vol. II. Hanover, 1797. pp. 1—81.

In passing over the medical writings and annals of all times, difficult dentition will be found to be almost unanimously acknowledged as a pathological phenomenon, and as the immediate cause

of

of many dangerous diſeaſes of children; and but few authors have hinted doubts againſt an opinion ſupported by the authority of its antiquity and by the force of prejudice. *Mercurialis*, who wrote about two hundred years ago, ſays in his work, *De puerorum morbis Francofurt*, 1584, p. 312, mentioning dentition: "Sed ſtatim nobis occurrit ſcrupulus non ſine animadverſione praelegendus, quomodo dentitio poſſit eſſe morbus; compertum enim eſt, naturam non intendere morbos neque favere, ſed dentitio eſt purum naturae opus, qua re non videtur morbus eſſe appellandus." However, he treats it as a diſeaſe, or, at leaſt, as pains concomitant with that operation of nature. In modern times alſo, ſome arguments have been ſuggeſted againſt the general prevailing opinion, by Cadogan, (on Nurſing, p. 31,) by Armſtrong (Diſeaſes of Children, p. 60,) to which his tranſlator, Dr. Schaffer, has added his own obſervations, tending to prove the inſufficiency of difficult dentition in producing ſome diſeaſes of the infantile age, that are commonly deduced from that cauſe. Notwithſtanding theſe doubts, Cadogan, Armſtrong, and Schaffer ſtill conſider it in too great a meaſure as the ſource of many infantile diſeaſes, not being able to prevail on themſelves totally to lay aſide that opinion, although they have found it in ſome reſpect inconſiſtent with their own obſervations and experience. Dr. Hecker, even though he denies thoſe dangerous ſymptoms to originate from the teeth cutting through, yet ſeems to regard dentition as a pathological phenomenon, aſcribing it to an irritation in the mouth, which neceſſarily muſt be cauſed by the teeth penetrating through.

During an extenſive practice of 30 years, Dr. Wichmann never could perſuade himſelf of the juſtneſs of the idea on difficult dentition; and confirmed by his experience in the doubts he had always entertained, he attempted at firſt to lay them before the public at large, however conſcious of the hazardous taſk of encountering a matter that has hitherto ſerved as an aſylum ignorantiae to many phyſicians, in explaining almoſt every morbific ſymptom by which infantile age is affected, and which ſo often proves fatal to it. The frequent mortality of children, occaſioned in the firſt years of life by convulſions, is moſtly attributed to the act of dentition, by which Hurlock (Practical Treatiſe upon Dentition, 1742) and other dentiſts, account for them. Berdmore too (on the Diſorders of Teeth and Gums, 1770, p. 192.) aſſerts, that more than half the children dying in the firſt two years of their life fall a ſacrifice to dentition; and in this the greateſt part of practitioners ſeem to have acquieſced, though none of them has ever given an exact account and explanation in what difficult dentition properly conſiſted, and the ſuppoſed morbid ſtate hence ariſing.

After theſe previous remarks, Dr. Wichmann proceeds to relate his opinion, which conſiſts in maintaining, that difficult dentition ought to be entirely exploded from the catalogue of diſeaſes, and that, as a pathological phenomenon, it exiſts but in the fancy of phyſicians. To ſupport, and to prove this aſſertion, Dr. W. enters upon a diſquiſition and examination of the different ſymptoms of difficult

ficult dentition, and ſhows how far they ſtand the teſt of criticiſm.—The ſymptoms ſuppoſed to attend difficult dentition may be divided into *topical* and *general.* Amongſt the topical ſigns have been enumerated,

1. *A ſwelling and hardneſs of the gums,* and this is looked upon as the ſureſt diagnoſis of difficult dentition, particularly when children are not yet able to expreſs the ſenſation of pain hence ariſing. But it is obvious to remark, that ſuch a ſwelling will be always found in the healthieſt children, who get their teeth in the eaſieſt way poſſible, when a tooth is preparing to cut through; and, beſides, if the ſwellings were in the leaſt of an inflammatory nature, it would certainly ſpread itſelf over the neighbouring parts; but it is limited and ſingle; elevations are only to be perceived, more or leſs, according to the number of teeth that are ready to break through. Inſtances of inflammation and of tenſion of the gums, and of a ſwelling of the cheek, which have been obſerved by ſome practitioners, at the time the dentes molares, or double teeth, are cutting, deſerve hardly to be brought forward as an argument for difficult dentition, and muſt rather be conſidered as anomalous caſes, deviating from the natural courſe of nature, which differ from dentition, commonly ſuppoſed to be difficult, as panaritium and pernio, and are only deſcribed as ſomething extraordinary.

Moreover, this ſuppoſitious inflammation of the gums ought to be particularly evident and ſtrong in the cutting of the eye-teeth, generally believed to be leſs eaſy, and, therefore, principally dreaded, if it were a ſignum pathognomonicum of difficult dentition; but on the contrary, at a cloſe inſpection, that ſuſpected red elevation will not even be found when thoſe teeth are almoſt hourly expected to get through at the end of the ſecond year, and even when ſome ſymptoms of illneſs or difficult dentition appear at the time they are cutting through. Redneſs of the gums is likewiſe a precarious ſymptom, as on the ſpot where a tooth is appearing, the gums are moſtly rather white than red.

2. *Pain of the gums* is counted amongſt the moſt important ſymptoms of difficult dentition, and though children are not always able to expreſs the ſenſation of it, and ſtill leſs to point at the place whence it ariſes, yet their exiſtence has not been in the leaſt doubted. However, children hardly complain of any pain on touching the gums; and even when they are of ſuch an age as to be able to expreſs themſelves, and to denote exactly the very ſpot where they feel the pain, particularly at the appearance of the eye-teeth, yet they never diſtinctly complain of pains at theſe ſuſpected places, though they are moſt cloſely examined, and moſt expreſſedly aſked for them. Beſides this, it ought to be preſumed that pains would be more perceptible in caſes where teeth break out in a preternatural way or direction; but this has not been found to take place when they grow one after another, forming a double row. Now, if in theſe caſes the jaw, the gums, the perioſtium, the nerves, or to whatever elſe difficult dentition has been attributed, ſuffer no affection at this time, at a ſtate ſo evidently violent and morbific, how can it be ſuppoſed to

to be so, when the growth of the teeth is slowly proceeding in a natural way, and without any violence? We find, moreover, recorded by some writers, that children come into the world with teeth that had cut through before their birth, where nothing may be thought prepared for such an action; and these children could have hardly lived if dentition was at all a violent and painful act.

Having pointed out how precarious and uncertain those symptoms prove to be, it only remains to examine, whether the gums or the periostium are really capable of giving pain; or whether, as it is commonly asserted, those parts are susceptible of such a tension as not to emit the teeth without a painful sensation. On this account practitioners never ought to be consulted, because they admit and maintain the presence of such a thing either without farther explanation, as Stoll says, "Dens exitum quaerens gingivam tendit;" or they derive the pain a compressione nervi, as Sauvage, well knowing that the gums suffer incision without causing pain. Some ascribe it to a consensus musculorum, as Girtanner, (on Dieases of Children, in German, p. 108.) to an irritation of the periostium. Under these circumstances recourse must be had to anatomy for determining satisfactorily the seat of pains, which are supposed to take place in dentition; however, we find almost all anatomists and physiologists agree, that the upper part of the gums is nearly deprived of sensation. J. Hunter, the first author on this subject, speaks, in his History of the Human Teeth, of the perforation of the gums, without in the least mentioning a periostium; and, in explaining the pains attending dentition, he believes that there is an irritation which produces a diminution and absorption of the gums, at a time when, according to others, a swelling of them is observed. Blumenbach (History and Description of the Bones, in German) says, that the teeth lie inclosed in a membrane, but he does not intimate that the upper part of them is also covered with it; and by the silence of other anatomists, as Prochaska, (Annotationes Academicae) Albinus, Meyer, Loder, it may be justly supposed that it does not exist after the birth. However, Haller expresses himself very distinctly, "Alveolus in foetu amprissime patulus circa conformatum dentem connivet dum pars vitrea et absque periostio eminet," (Element. Physiologiae, T. iv. p. 23.) Soemmering (on the Structure of the Human Body, in German, 1791, T. i. p. 207.) says, "While the roots of the teeth are growing longer, the teeth are protruded, and *bare of periostium*, they penetrate the gums."—A still greater authority for this opinion is Alexander Monro, one of our first writers on osteology, who rejects the existence of a periostium with plain words; "without the gums the teeth are covered with no membrane," (Anatomy of the Bones, Edinb. 1748, p. 149).

After giving these proofs of the non-existence of a periostium, it is needless to dwell any longer upon this subject; and it remains now to be considered, whether the gums are thus pierced and irritated by the teeth as to produce those dangerous symptoms of difficult dentition. It has already been intimated, that the best and latest physiologists agree in denying almost all sensibility to the

gingiva:

gingiva: but this is farther confirmed by the teſtimony of ſurgeons, who think the inciſion of the gums an operation attended with no pain, and conſequently the gingiva is nothing but a ſenſeleſs ſpungy ſubſtance, which eaſily yields to the preſſure and growth of the teeth. Hunter himſelf attributes but little ſenſibility to the gums, remarking, that before the teeth appear they ſerve in their place, which is likewiſe the caſe with old people. Another inconſiſtency is, that it is propoſed, by way of leſſening the danger of dentition, to make the inciſion, which is ſuppoſed to be attended with no pain, and yet a pain in the gums is taken to be the cauſe of dangerous ſymptoms! If then, thoſe diagnoſtic ſigns, ſo generally conſidered ſtrong and inconteſtable, are proved to be uncertain and illuſive, and if at the ſame time no pain can ariſe from the gums, the ſuppoſition of which has been employed as a cauſe of all the different ſymptoms of difficult dentition, it muſt be candidly allowed that this pathological explanation is deficient and uncertain.

3. *Salivation* is another ſymptom attending dentition, on which phyſicians rely ſo much, that at perceiving this in a child they do not require any other ſymptom, and hardly think it worth while to examine the mouth; but if they did, they would very frequently find ſalivation was occaſioned by aphthae or the thruſh, of which it is, as well as pains, a common ſymptom; and though they are not always met with, yet they are more frequently the cauſe of this ſalivation than has hitherto been thought; and in caſe they ſhould not be there, it is more natural to derive the ſalivation from a ſlight irritation of the ſalival glands than from dentition.

There is ſtill another ſymptom likewiſe apt to deceive, and that is, *when children often move their hands towards the face.* Of many inſtances, the following may prove the truth of its being fallacious. A child, two years old, had been ſickly about a fortnight from an unknown cauſe; it loſt appetite, ſleep, and became feveriſh; it ſhrieked ſometimes, and often brought its hands towards the face, when Dr. W. was ſent for. On undreſſing and examining the child, to diſcover the cauſe, an excoriation in the ear was found, which being removed the child got quite well again. In diſeaſes of children, therefore, nothing is more neceſſary than exact examination of the whole body, which will frequently, if not always, ſhow how ſmall a ſhare dentition has in producing them, and how eaſily one may be deceived in thinking that the pain ariſes from the place to which children appear to point. Of the numerous inſtances by which this may be demonſtrated, a very intereſting obſervation of Dr. Lodemann, of Hanover, deſerves to be mentioned. A boy, five months of age, became ſickly, cried often, without having any fever, and the cauſe was aſſigned to teething. Soon after convulſions, triſmus came on; he loſt the power of ſwallowing; nothing but external remedies could be applied, and he died after forty-eight hours. In diſſecting the body, the true cauſe was diſcovered to be a preſſure of the left teſticle in the abdominal annulus.

After having ftated what can be faid of the topical figns of difficult dentition, Dr. W. proceeds to confider the general fymptoms and difeafes afcribed to it. When all thefe difeafes are impartially examined, they will be found not to differ in the leaft from other affections, daily obfervable in the firft four months of life, where teeth are not yet fufpected, and after the fecond year when moft teeth have appeared. They feem, therefore, to admit a more natural explanation: By confidering them as idiopathick (aphthae) and diarrhoeas, colics and vomition are much eafier explained from a corrupted milk, and an inflammation of the eyes from a fcrophulous difpofition. It feems, likewife, inexplicable how a kind of dyfentery can originate from dentition, as is afferted by medical authors, particularly when no pain in the mouth, and other topical fymptoms are perceived, and when fimilar evacuations occur in children of five or fix years of age. It is more probable that it arifes from a topical caufe, or any other lefs remote from the inteftines. A ftrangury and retention of urine are alfo fuppofed to be confequences of dentition in female children; but they really are only owing to an excoriation and exulceration of the genitals, and eafily yield to a topical treatment. A lethargic ftate is more probably caufed by a hydrocephalus internus than by dentition. The gonorrhoea, which is fometimes obferved at the time of dentition, is nothing elfe but an accumulation of matter, fecerned by the glandulae tyfoni, and is a topical affection foon removed by injection under the prepuce. With refpect to the fever incident at the period of teething, it may be obferved, that many circumftances can occafion it, as children, even in that age, are fubject to every febrile affection; and Rufh has feen the yellow fever in a child of four months, without any neceffity of affigning dentition as the caufe of it; and it is fcarcely pardonable to neglect other remedies with the idea of its being merely the confequence of the irritation of the teeth; and befides, it does not appear that it is removed when the teeth have cut through; and frequently, when the fever has been attributed to dentition, no teeth have appeared. There are alfo fome febrile affections remarked as fymptoms of a latent fcrophulous difpofition, and thefe are frequently miftaken for the teething fever.

Of all morbid affections by which children are afflicted, during the time they are cutting their teeth, none have been fo generally, and without exception, attributed to dentition as convulfions; but when we confider that moft probably they are owing to caufes more frequently occurring, to a momentary corruption of the milk from paffions of the mind, to other difeafes of the nurfe, to the refloration of fluxus menfium, &c.; and when we farther confider that all important difeafes generally begin with convulfions in the infantile age, it may be concluded how precarious the idea will prove in the practice. The following cafe may, moreover, prove the fallacy of this fymptom: – A healthy boy, feven months old, was feized with convulfions and pains, which recurred in a fhorter or longer fpace of time. Gentle purgatives, clyfters, and blifters behind the ear, removed

removed theſe affections, but after four weeks no elevation or penetration of a tooth was to be perceived.

Such are the ſymptoms, the pathology, and diagnoſis of difficult dentition, of which it has been attempted here to point out the deficiency and fallacy. In ſurveying the method of cure delivered by medical writers, we meet with the ſame inconſiſtency, and the groſſeſt empirical uncertainty. Hurlock has collected all ſuperſtitious remedies that were in uſe in his time, and has beſides enriched it with the hare's-brain, which was employed to ſoften the gums or to promote the growth of the teeth. Hard and ſoft things were indiſcriminately given to children to bite; and, in ſhort, remedies were employed which only ſuperſtition could have invented, and ignorance and indolence kept up. But of all remedies, none has been ſo generally received and ſupported by the authority of practitioners as the inciſion of the gums, of which Ambroſius Paré, an eminent French ſurgeon, of the ſixteenth century, ſeems to have been the inventor. The uſe of this operation was afterwards much confirmed by Hurlock, though rather too far extended by him, as he applied it at the age of three years; and once even when the child was but ten days old. Berdmore, a known dentiſt of his time, and other eminent practitioners, have adopted it. Underwood (Diſeaſes of Children, 1784, p. 95) recommends it very much as being attended with no pain, though pain and inflammation are ſaid to take place at this very ſpot; and, beſides this, he derives it from the perioſtium or membrana nervea, covering the teeth immediately under the gums; but it has been ſufficiently ſhown that no ſuch thing exiſts.

[To be concluded in our next.]

Elemens de Myologie et de Syndeſmologie, par Thomas Lauth, 1 *vol. à Bâle, chés Decker, à Paris; chés Koenig, et à Straſbourg. VI. année de la Republique.* 1798. pp. 204.

AMONGST the number of anatomical compendiums that have been publiſhed in different countries, the preſent deſerves to be recommended on account of its accuracy, conciſeneſs, and the order which the author follows, as well on account of containing a complete literature, and references to the beſt figures. The whole is divided into ſeveral lectures, of which this volume contains but eight, beſides an introduction, in which ſome general remarks on the ſtructure, action, diſtribution, and denomination of the muſcles, on the folliculi mucoſi, and the ligaments, on the literature of this branch of anatomy, are previouſly explained. The redneſs of muſcles, which the author conſiders as eſſential to them, merely depends upon the red part of the blood contained in their veſſels, and eaſily diſappears by putting them in water for ſome time, and beſides the muſcles of animals, without red blood, (animalia ex ſanguia,) as inſects, are white; that therefore the red colour cannot be conſidered as an eſſential property of muſcular ſtructure. The word *musculus* he thinks rather to be derived from *μυειν* (contrahere) than

than from μυς, as it is generally ſuppoſed. In deſcribing the muſcles, he proceeds almoſt according to *Albinus's* method, though it ſeems a little inconvenient, that the deſcription of thoſe muſcles which extend to different parts, is ſeparated in ſuch a way, that, for inſtance, the greateſt part of the latiſſimus dorsî is deſcribed with the muſcles of the back, but its ſmaller portion, and tendon with thoſe of the arm. It is, however, very uſeful to the beginner, that the author has prefixed to the deſcription, the beſt mode of preparing each part anatomically in the following order:—*preparation, ſynonimous terms, ſtructure and inſertion, and uſe.* In mentioning *Veſalius's* work, he ſupports an opinion of *Albinus*, that the plates to it were not done by *Titian*, but by *Jean Etrenne*, becauſe he thinks it not likely that *Titian* ſhould have attended to anatomy at the advanced age of 62, when young *Veſalius* was publiſhing his work; being, beſides, very much occupied with pictures, that were ordered to be made by him; and it is moreover obſerved, that great painters are ſeldom able to draw objects of natural hiſtory and anatomy with accuracy, which can only be acquired by continual practice.

Chemiſhes Handwoerterbuch, &c. i.e. *Manual Lexicon of Chemiſtry, compoſed after the lateſt Diſcoveries, by* Dr. DAV. LEWIS BOURGUET, Profeſſor of Chemiſtry, at Berlin. *With a Preface of* Dr. SIGM. FREDERICK HERMBSTAEDT. Vol. 1. A—E. 1798. Vol. 2. F—K. 1799. Berlin for Bemigke. Pr. 4 rixdollars, or about 16s.

ALTHOUGH we poſſeſs in the Chemical Dictionary of Macquer, a work which is full of the beſt chemical information, and for the time in which it was written, extremely well performed, and calculated to give a complete and general ſurvey of chemiſtry, yet the aſtoniſhing progreſs which that ſcience and art has made of late, and which is almoſt daily increaſing, ſeems to have rendered it neceſſary to compoſe a new work of the kind, whoſe chief object of utility and accuracy, would of courſe conſiſt in having collected all diſcoveries and facts in chemiſtry that are known to the preſent times. Mr. *Bourguet* has therefore a claim to the praiſe of the public for having undertaken this taſk, of which, as far as we are able to judge, he has hitherto ſufficiently acquitted himſelf. However, it would not have been amiſs, if in ſome reſpects he had attempted to be more conciſe, which we think is a neceſſary requiſite for a work, whoſe object is to convey information in an eaſy way. More care ought alſo to have been taken in the correction of the preſs.

Deſcription

Description of Mr. HUMBOLDT's *Anthracometer, or an Instrument for measuring the Quantity of Carbonic Acid contained in the Air.*

[With an Engraving.]

THIS instrument consists of a very strong glass tube, from 3 to 4 lines wide, and about 12 inches long; and ends in a globe of 1.2 or 1.3 inch in diameter. The lower three inches of the tube are bent in such a way, that the distance of the globe from the cylinder is no more than 6.3 inches, for the sake of putting it into a narrow glass of water. The tube *a b* must be of equal width, but it is indifferent whether it be wider in *c* or *d*; however, it is better when the capacity of the whole cylinder does not exceed 2 or 2.5 cubic inches. In *e* it is divided in two sections, of which the upper one must remain 7 inches long; both parts are joined by a metallic cylinder, so that they may be unscrewed; but no water must get through. The upper end of the glass tube is cemented into a metallic cylinder, 6 inches, high *f*, which has, on its outside, nine small worms of a screw. Its mouth is conically turned out, and into this a muscle-shell-like valve is put, of the thickness of one or two lines. Another metallic cylinder, *h*, 5 lines high, shut by a plate above, *k l*, and excavated as the box of a screw, exactly fits the tube as a cover. To increase the pressure, the plate *k l* has a hole in the middle, and another screw presses the valve to the mouth of the tube.

The use of this instrument is very simple. Fill it with *liquid caustic ammonia*; pour out of the tube, *a e*, as much as you are going to examine of the air, and mark with a pair of compasses the length of the column of air on a scale; then shut the valve, and let the air go into the globe; and the carbonic acid contained therein, will, on account of the greater surface it finds, combine itself entirely with the caustic ammonia. The latter will begin to sink in the tube; and when this process is over, open the valve and fill the whole tube with it. Let the air from the globe pass into the tube again, which is then unscrewed at *e*, under water; and the upper piece must likewise be immersed till the water is equally high at the inside and the outside. The remaining part of the air, substracted from the above quantity, shews the proportion of carbonic acid. Instead of the ammonia, kali may, perhaps, be preferable, as it is more easily prepared. The instrument may likewise be used for measuring oxygen gas.

Remarks on the Application of cold Water in Amaurosis and Weakness of Sight; by Dr. BEER, *Occulist at Vienna. (Extracted from* Arnemann's Magazine for Surgery, *Vol. II. No. I.)*

Cold water is undoubtedly one of the most powerful and surest remedies against the weakness and inactivity of the vessels and optic nerves, which generally remain after a strong congestion of the blood towards the head and eyes. It is therefore employed with the greatest success in that species of amaurosis, caused by a congestion

gestion of blood to the head, after sufficient quantities of blood have been taken away by a general and topical venæsection, to remove the subsequent distention and weakness of the vessels. How useful the external application of cold water often proves to be in amauroses, originating from a general debility of the body, or a topical weakness of the eyes, is known to every practitioner.

Persons who have suffered vehement inflammations of the eyes frequently retain a dim and weak sight, though no visible fault can be discovered in the eyes themselves. They sometimes are entirely cured in a few days, by the external use of cold water. It likewise happens after the extraction of the cataract, that the operated persons complain of black serpentine lines, marks, and dust, floating before the eyes, and hindering the sight; which symptoms always yield to the use of cold water.

The application, however, of this excellent and simple remedy requires some caution. As long as any symptom of congestion is perceived, it should be carefully avoided. The patients, indeed, will feel a momentary relief and amendment in the sight soon after the application, but a great tension in the eye-ball, a decrease or entire loss of sight, and sometimes vehement ophthalmies follow the inconsiderate use of cold water. When, on the contrary, the face of the patient is pale, and the white of the eye not red or yellowish red, but when only a few turgescent blood vessels are remarked on it, when motion causes no head-ach, nor increases the dimness, when sparks or other shining corpuscles appear no more before the eyes, and the patient perceives no tension, or any other particular sensation in moving the eyes, cold water may be used with safety and advantage.

In applying it, a gradual proceeding is to be observed. At first, head and eyes should be covered with a cloth dipped in cool water, which must be frequently repeated. Some days after they may be bathed with colder water. After having performed this for eight or ten days, it may be applied in a more powerful way by means of a syringe, with which the water is brought to the eye-lids, eye-brows, temples, and, if the patient can bear it, to the eyes themselves.

The usual mode of applying the water in these cases by means of small cups of glass, or earthern ware, can never have any effect, by the way of coldness, because the water gets too soon, in this way, a considerable degree of warmth; but the other application has the double effect of bringing the water sufficiently cold to the part, and by causing an efficacious commotion in the eye itself.

To apply the water in this manner with greater ease and more advantage than by a syringe, a very convenient instrument has been invented by Dr. *Beer*, which is very well adapted to the purpose. (see the plate.) The whole is made of copper or tin, and consists of a box, in which is inclosed a smaller vessel, made of pewter, *F*, which contains about five or six pounds of water. The space *G*, between the two vessels, is filled with broken pieces of ice, and the degree of cold may be increased according to circumstances, by

by adding ſalt or ſal ammoniacum to it. The tube is divided into four parts, which are well joined by ſcrews, *D*, by which means they can be conveniently put into the box, when the inſtrument is not wanted. By turning the cock, *E*, the water may be ſtopped, or by opening it more or leſs, the force of the fall of the water be increaſed or diminiſhed, as it ſeems proper. (For this purpoſe, it would be very convenient, if the ſpout of the tube *I*, could be unſcrewed, ſo that different ſpouts with larger or ſmaller openings might be put on, as the former would tend to leſſen the force of the water, and the latter to increaſe it. Another ſpout, made like the roſe of a watering-pot, and perforated with many holes, would be well calculated to give a greater ſurface for the water to act upon.)

The whole inſtrument is about five feet long, and may be ſuſpended in ſome convenient place of any apartment; but it ſhould be high enough that the patient need not, in uſing it, bend the head too much down. The lid, *C*, is faſtened on the veſſel in *B A*, as appears from the plate.

In obſtinate caſes of Amauroſis, where cold water is preſcribed, it may be uſed as a dropping bath, by ſcrewing the ſpout, *H*, to the tube, inſtead of the curved end. The cock may be turned in ſuch a way as only to emit the water by drops, which fall either on the eye-brows or temples; and the effect of this powerful remedy can be leſſened or ſtrengthened, according to the height from which it is applied. The commotion and pain, cauſed by this mode of application, is generally ſo vehement, that many patients will not bear it. However, its efficacy is very great, and caſes, where it proves without avail, muſt be conſidered as doubtful and moſtly incurable, and very rarely yield to other exciting remedies, as electricity, &c.

The uſe of this convenient inſtrument might be made more general and common in bathing and ſtrengthening the eyes, and would do a great deal of ſervice, particularly to thoſe whoſe ſight is weakened by reading, lucubration, or by any other means. To theſe it would prove a very uſeful and commendable piece of furniture, and for that purpoſe the above mentioned alterations in the ſpout would, perhaps, be requiſite; the ſmaller veſſel, *F*, might then be taken out, as the degree of cold need not be ſo great for common purpoſes.

PHARMACEUTICAL PROCESS.

To obtain acid of tartar in a very pure ſtate, Mr. *Buckholz*, of Erfurt, uſes the following method, founded upon experiments.

Take of finely pulveriſed cryſtals of tartar 6lb. and after boiling them in a proportionate quantity of ſpring water, add by degrees, continually ſtirring the mixture, pure, finely pulveriſed, and well waſhed chalk, till no efferveſcence be perceived, and till it have

no effect on blue paper. Twenty-six ounces of chalk will be required on an average for 6 lb. of crystal of tartar. Pour off cautiously the salt solution, which contains tartarus tartarisatus, and wash the remaining tartareous selenites till it become quite free from salt and other heterogenous particles. Mix 1lb. of these selenites with 6 oz. 7 dr. and 50 gr. of white pure sulphuric acid, 1,840 specific weight, which has been diluted before with 6 or 8 times its weight of pure water. Let the mixture now be digested for three days, frequently shaking it. After this, let the tartareous acid be strained through a clean bag, and free it from selenites by expression and washing with cold water. Let all the liquors evaporate by a gentle fire. After being cool, the acid is to be dissolved in a little water, and the solution filtered, by which means a considerable quantity of selenites is separated, which is washed till it becomes insipid. The filtered liquors are then altogether evaporated to the consistence of a thin syrup, whereby again a small portion of selenites separates, which must be purified from the acid by filtering and expression. All the acid liquor is now exposed in a moderate warm place to a slow and quiet evaporation. By this mode Mr. *B.* obtained the finest white crystals. The liquor remaining over them must be poured off, and the crystals put upon paper, in which there is no glue, till they become dry. This last liquor, which is a little yellowish, is either gently evaporated, dry tartareous acid is got, or some nitric acid is added to it, till it be discoloured.

From the above quantity Mr. *B.* obtained 35—36 ounces of tartareous acid; 2 or 3 ounces included, which were absorbed by the paper. The acid prepared in this way was quite free from sulphuric acid. (*Tromsdorf's Journal of Pharmacy, No. I. in German.*)

MEDICAL

MEDICAL AND PHYSICAL INTELLIGENCE.

Prof. Wurzer, of Bonn, relates a case of venereal disease, where nitric acid was employed with the greatest success. A young man being apparently cured of local venereal affections, had soon after venereal ulcerations in the mouth, and at the same time, an exanthematous eruption breaking out over the body, particularly in the face. After having undergone six salivations, the chancres healed, but the eruption, which covered the whole body, continued to increase, and began to ulcerate at different parts of the body. His constitution was so extremely debilitated, that Prof. W. to whom he applied for advice, was obliged to strengthen him by a proper diet and regimen and corroborant remedies, combined with such as are recommended in similar cases, as guaiacum, sarsaparilla, &c. Although his strength was somewhat re-established, yet no change was brought on in the eruptions and ulcerations, and Prof W. began now to give him those mercurials which are less apt to produce salivation; as, sublimate and mercurius cinerus, or solubilis Hahnemann; but after taking only one or two doses, a vehement salivation came on, which again weakened him so much, that he was obliged to leave off taking mercurials, particularly as the symptoms increased, and most excruciating pains of the bones (dolores osteocopi) were now produced, of which he felt little before.—Under these circumstances, Prof. W. had recourse to Nitric Acid, of which he gave him one drachm diluted with two pounds of water, to be taken in twenty-four hours, with a decoction of sarsaparilla. Having continued this for some time, the eruptions and ulcerations began to dry, the pains in the bones ceased by degrees, and in the space of three weeks the patient was quite recovered; by strengthening medicines and cold baths his health was entirely re-established. Prof. Hufeland, in whose Practical Journal, Vol. iv. No. 19, this case is related, remarks, that nitric acid, according to his experience, sometimes proved of great benefit in affections which are rather consequences and morbid remainders of venereal disease, and not any longer depending on the poison; but that he never saw it of any use in a true venereal complaint, while the poison was still active.

Dr. Hargens, of Kiel, has made some observations, which seem to confirm the great use of alkali in convulsive affections.—A child, three years of age, was vehemently afflicted with convulsive fits, particularly in the left side; against which, flores zinci, musk, extractum hyosciami, clysters of assafœtida, anodyne inunctions with opium, and blisters, were all applied in vain. The oleum tartari per deliquium, or aqua kali preparati, was now given

to twenty-five drops every five minutes. Immediately after the fir dose, the convulsions diminished; and after the second, their violence had entirely ceased. —— Another child, nine months old which suffered dreadful convulsions, took fifteen drops every five minutes, and was almost instantly relieved by it. It proved likewise of very great service in other spasmodic affections; and, at least, as a palliative remedy in cardialgic paroxysms, particularly those of the hysteric kind, and in chlorotic constitutions. (*Hufeland's Practical Journal, Vol. viii. No.* 1.)

Dr. Gebel relates an instance of the great use of the extractum aconiti in a vehement rheumatic cardialgy, given in a dose of two grains every morning and evening, after which it ceased immediately, though large doses of opium had been applied before without any effect. This remedy, which seems to be neglected in this country, has still kept its reputation amongst the physicians of Germany, principally in obstinate rheumatic complaints. (*Ibid.*)

In the same Number of that Journal, we find the use of Mercurius sublimatus corrosivus, very much recommended in obstinate amaurosis, and confirmed by two cases. The first was a patient much affected with rheumatism, of which he was relieved by guaicum, aconitum, sulphur, antimonials, &c.; but the amaurosis suffered no change by these remedies, and rather got worse. After having tried oleum cajeput, valerian, electricity, &c. without effect, the patient was ordered to take the corrosive mercury in the following way:—℞. *Mercurii sublimat. corrosiv. gr. ij. solve in aether vitriol.* ℥ij. of which to take 10 drops every morning and evening in half a cup of warm milk. When he had used about 12 grains of mercury, the sight was so far re-established, that he was able to read and write. Though he was perfectly well, it was thought proper to continue the mercury for some time; but as the sight seemed to decrease by this continuation, it was left off. It is remarked by farther experience, that probably as soon as a certain dose has had its effects, farther benefit is not to be expected from the continuation of this remedy, but rather the contrary. Another patient, quite blind of both eyes, used it for six weeks, in which time he entirely recovered the sight of the left eye, though no change was produced in the right; but it was not thought proper to continue the remedy on the above account. It is, however, proper to observe, that this powerful remedy should be used with caution: it seems particularly indicated, when any suspicion of a venereal cause exists, or in rheumatic amauroses, as it has been lately recommended in such complaints. (*Ibid.*)

In the same Journal the following fact is mentioned, which deserves the attention of our readers.—An anatomical preparation of indurated glands from a very scrophulous subject being put into spiritus vini, was found some time after to be incrusted with saline crystals. To prove that they really came from the preparation, and

were

were not contained in the ſpirit, it was laid into other ſpiritus vini wherein likewiſe cryſtals appeared upon it, but none upon another preparation, which was put into the former ſpirit. By a chemical analyſis, thoſe cryſtals were found to conſiſt of ſaccharic acid. This inſtance ſhews what changes are produced in the chemical mixture of the humours of the body by the morbid affections of it, and ſeems to prove, that a particular corruption of the lymph, of an acid nature, takes place in the ſcrophula, which, though originating from a vitiated action of the lymphatic ſyſtem, muſt be conſidered as the prime cauſe of many otherwiſe inexplicable ſymptoms of that diſeaſe, and which ſeems, at the ſame time, to explain the uſe of abſorbent and alkaline remedies here. (*Ibid. vol.* vii. *No.* 1.)

Citizens Buniva and Vauquelin have, in the analyſis of the liquor amnii of a cow, diſcovered that it contains a peculiar acid, which they call *acide amniotique.* We ſhall, perhaps, communicate to our readers, the paper on this ſubject, which is printed in the *Annales de Chemie, Ventôſe, An. viii.*

We have already mentioned in one of the Numbers of this Journal, that the late Mr. Girtanner, of *Gottingen,* meant to have diſcovered the analyſis of the azote. In a letter to Cit. *Van Mons,* he gives a ſhort account of it. Azote is now, according to him, a compound ſubſtance conſiſting of hydrogen and oxygen in the proportion of 0,21 of the former, and 0,79 of the latter; and conſequently, nothing but an oxyde of hydrogen. Our eudiometrical operations are erroneous with reſpect to the azote, becauſe this is but a production, and did not exiſt in the air before the experiments were made. Alum and aluminous earth are the ſubſtances which moſt eaſily change atmoſpheric air into azote. It is produced by reſpiration, the oxygen, of which a part is always condenſated in the lungs, combining itſelf with the hydrogen, and changing it into azote; which however, more viſibly takes place by bringing hydrogen in contact with wet aluminous earth. We have to expect a treatiſe upon the ſubject. (*Ibid.*)

Mr. Eckeberg gives a more accurate account of a blackiſh ſtone found at Ytterby in Sweden, and of the new earth contained therein, called Ytterearth. This foſſil was firſt analyſed by Mr. Gadolin, but he does not mention the feld ſpar which Mr. Eckeberg found mixed in it. From 100 parts of the foſſil he obtained 47 ½ parts of that new earth. It is ſoluble in all acids, to which it imparts a ſweetiſh taſte; cauſtic alkalis have no effect upon it. It forms with ſulphuric acid an eaſily ſoluble ſalt, which ſoon ſhoots into cryſtals, and keeps its acid entire. (*Swediſh Tranſactions, Vol. xviii.* 1797.)

Prof. Parrot, of Riga, has made ſeveral experiments, by which he attempts to ſhow, that coal is a compound ſubſtance,

that

that carbonic acid is only contained accidentally in it, as in chalk and lime; that the true carbon differs from it, and is either nothing but hydrogen, or a combination of hydrogen and azote, and consequently that there exists no proper hydrogen, but only carbon, and that water is now composed of carbon and oxygen. (*Voigt's Magazin for Natural Science, Vol. ii. No. 1, p. 131.*)

Dr. Seybert, of Philadelphia, writes in a letter to Prof. Blumenbach of Gottingen, that the Philosophical Society of Philadelphia has now obtained a large collection of bones from the western country, of that prodigious unknown animal, called *Mammut Ohioticum*, which will throw a new light upon its osteology and form. It is remarkable, that some bones of the posterior extremities have a great likeness to human bones, as the patella, tibia, the tali calcaneus, and the os cubiforme. (*Ibid.*)

Prof. Linck, of Bostock, relates in a letter to the same Professor, a rare instance of a mule having become pregnant in the province of Algarbia, in Portugal, where it was shown for money as a great curiosity, first pregnant, afterwards with the young one. Instances of mules propagating their own species are extremely rare; however, they can be used as a strong argument against a notion adopted by Ray and Buffon, that those animals which are capable of producing with one another young ones, and are possessed of fecundity, belong to one and the same species; because, if this was a general rule, the horse and ass ought, according to those rare instances, to be considered as of the same species, however different they are in their external and internal structure. (*Ibid.*)

Dr. Meyer, of Berlin, relates an experiment, by which he was convinced that iron particles entered into the blood with the chyle. He gave at first to a dog five grains of limatura martis per day, and afterwards the same portion in the morning and evening. Having continued this for eight days, the dog was killed, an hour and a half after he had taken the last dose. A ligature being applied to the ductus thoracicus, Dr. M. obtained from it about one drachm of chyle, in which not the least iron particle could be traced, either by any reagency or by combustion. All the fluid found in the stomach and intestines, shewed that it contained iron, by a black precipitation, when mixed with a solution of volatile liver of sulphur. To be still more sure of the non-existence of iron in the chyle, in case it should be present in such minute portion as not to be affected by reagency, he mixed a small quantity of a very diluted solution of vitriolum martis with the chyle, and tried it with the volatile liver of sulphur, when a black precipitation was immediately produced. *Reil's Archives for Physiology, Vol. iv. No. 3.*

Dr.

Dr. SCHMIDT, of Osnabruck, who has related that curious instance of abstinence from food and drink, which we communicated to our readers in the last Number of this Journal, acquaints the public, that the whole case is discovered to be a most subtle imposition, whose proceeding and discovery he is going to publish in a pamphlet; and we shall not omit to give an account of it here, as a circumstance very interesting with respect to Psichology, and also as an instance how cautious physicians always ought to be in similar cases, and how little the most appearing circumstances are to be trusted.

Mr. PASUMOT, mentions in his Voyages Physiques dans les Pyrénées, 1788 and 1789, Paris, 1797, that on his journey to Gavarin, he has heard of a family being extinguished at Visos, about seventeen years ago, whose members were all eight feet high, and amongst them a man reached the age of 110 years.

Dr. WOLCOT, (the admired satirist, Peter Pindar) has it in contemplation to publish a Treatise on the general Causes of Deafness, with the modes of cure. Such a publication is certainly a *desideratum*, as no minute and satisfactory account of the complaints of the Ear has hitherto made its appearance.

Many unfounded reports having been circulated, which have a tendency to prejudice the mind of the Public against the Inoculation of the Cow-Pox; we, the undersigned Physicians and Surgeons, think it our duty to declare our opinion, that those persons who have had the Cow-Pox, are perfectly secure from the infection of the Small Pox, provided such infection does not exist in the system at the time of the inoculation for the Cow-Pox.

We also declare, that the inoculated Cow-Pox is a much milder and safer disease than the inoculated Small-Pox.

William Saunders, M. D.
Matthew Baillie, M. D.
Henry Vaughan, M. D.
Maxwell Garthshore, M. D.
John Coakley Lettsom, M. D.
James Sims, M. D.
John Sims, M. D.
William Lister, M. D.
Robert Willan, M. D.
C. Stanger, M. D.
Alexander Crichton, M. D.
Thomas Bradley, M. D.
Thomas Denman, M. D.
John Squire, M. D.
Richard Croft, M. D.
Robert Batty, M. D.
R. J. Thornton, M. D.
Richard Dennison, M. D.
Henry Cline.
Edward Ford.
Astley Cooper.
John Abernethy.
Joseph Hurlock.
William Blair.
Samuel Chilver.
J. M. Good.
James Horsford.
Francis Knight.
James Leighton.
James Moore.
Thomas Paytherus.
Thomas Pole.
J. W. Phipps.
John Ring.
James Simpson,
H. L. Thomas.
Jonathan Wathen.
Thomas Whateley.

NEW

NEW MEDICAL PUBLICATIONS IN JULY.

Obſervations on the Cow-pox, by William Woodville, M. D. Phyſician to the Small-pox and Inoculation Hoſpitals. Phillips.

Reflections on the Cow-pox, illuſtrated by Caſes to prove it an abſolute Security againſt the Small-pox; in a Letter to Dr. Jenner from William Fermor, Eſq. Robſon.

Noſology; or, a Syſtematic Arrangement of Diſeaſes, by Claſſes, Orders, Genera, and Species, with the diſtinguiſhing Characters of each, and Outlines of the Syſtem of Sauvages, Linnæus, Vogel, Sagar, and Macbride; tranſlated from the Latin of William Cullen, M D. 6s. bds. Kay.

Reſearches, Chemical and Philoſophical, chiefly concerning Nitrous Oxyde, or dephlogiſticated Nitrous Air, and its Reſpiration; by Humphrey Davy, Superintendant of the Medical Pneumatic Inſtitution, 8vo. 10s. 6d. boards. Johnſon.

The Doctrine of Phlogiſton eſtabliſhed, and that of the Compoſition of Water refuted; by Joſeph Prieſtley, LL. D. 2s. 6d. Johnſon.

NEW MEDICAL PUBLICATIONS IN GERMANY.

Syſtem der Chirurgie, i. e. Syſtem of Surgery; by J. ARNEMAN, Profeſſor at Gottingen, Vol. I. with plates. Gottingen, 1799, 8vo. pp. 350. Vandenhock and Ruprecht.

Verſuch einer Pragmatiſchen Jeſchichte, i. e. Eſſay of a Pragmatical Hiſtory of Medicine; by KURT SPRENGEL; fourth Volume, 1799, pp. 564, 8vo. price two rix-doll. or about 7s. Halle, for Gebauen.

Bertrage zur Medicinischen Klinik; i. e. Contributions to Clinnick Medicine, collected on his Travels through Germany, Swiſſerland, and France, by ERNEST HORN, of Brunſwick, Vol. I. 1800, pp. 600, 8vo. price one rix-doll. twenty gr. or about 6s. Brunſwick, for Reichard.

Elementarlehre der Organiſchen Natur; i. e. Elementary Syſtem of Organic Nature; by Dr. FRANCIS JOSEPH SCHELVER, firſt Volume, Organomie, 1800, pp. 103, 8vo. price twelve gr. or about 2s. Gottingen, for Dieterich.

To CORRESPONDENTS.

We again requeſt our Correſpondents to be particular in their manner of writing proper names of perſons and places, as ſeveral inaccuracies have ariſen from a want of diſtinctneſs in the character.

Communications have been received from Dr. Cappe, Dr. Gillespie, Dr. Sugrue, and Mr. Guthrie.

ERRATA.

No. XVII. Page 6, line 10 from the bottom, dele *to be*, and read "*where the pupil is quickly dilated.*"

Page 7, line 2, for *irritability*, read *immobility*.